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MINERALS, NEW ARRIVALS.

Visitors to the Columbian Exposition who saw the remarkable exhibit of minerals in the New Jersey department of the Mining Building will be pleased to know that on Oct. 8th, Dr. Foote purchased the entire private collection that was there exhibited. This collection contains the finest specimens ever found of several species.

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Chalcophanite, museum specimens, $3 to $5; smaller, 10c. to $2. **Amphibole,** fine groups, $1 to $7.50.

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Rutile, *red* from Chester Co., Penna. We have recently purchased the finest series of these exceedingly beautiful geniculated crystals that has ever been offered for sale. Fine specimens, $2 to $15; smaller, 5c. to $2.50.

Black Arkansas **Rutile** for the Chemical laboratory, 15c. per lb. *Red* Chester Co. **Rutile,** the kind preferred by the Dentists, $1.25 to $2.50 per lb.

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SEND FOR A CIRCULAR containing a descriptive list of a very large number of species and varieties recently received.

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BOOKS.

Many would prefer a rare and useful scientific book to any other gift. Thousands of these can be supplied on demand, as we are constantly purchasing libraries. The Geological and Mineralogical library of the late Prof. F. A. Genth, just purchased, contained many rare and valuable volumes, such as the following: *Stephenson,* Geology of Georgia; *Tuomey,* Geology of South Carolina; *Lieber,* Geology of South Carolina, *Rossmässler,* Iconographie der Land and Susswasser Mollusken.

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[THIRD SERIES.]

Art. LI.—*On Variations and Mutations;* by W. B. Scott.

Very interesting and instructive analogies exist between the history, aims and methods of comparative philology, on the one hand and those of animal morphology, on the other. In both sciences the attempt is made to trace the development of the modern from the ancient, to demonstrate the common origin of things now widely separated and differing in all apparent characteristics and to establish the modes in which and the factors or causes by which this evolution and differentiation have been effected. At the present time morphology is still far behind the science of language with regard to the solution of many of these kindred problems and can hardly be said to have advanced beyond the stage which called forth Voltaire's famous sneer: "L'étymologie est une science où les voyelles ne font rien, et les consonnes fort peu de chose." Of the animal pedigrees, now so frequently propounded, few have any better foundation than the "guessing etymologies" of the last century and for exactly the same reason. Just as the old etymologists had no test to distinguish a true derivation from a false one, except a likeness in sound and meaning in the words compared, so the modern morphologist is yet without any sure test of the relationships of animals except certain likenesses or unlikenesses of structure. How much weight is to be allowed a given similarity and how far this is offset by a dissimilarity which accompanies it, we have, as yet, few means of determining and have still to discover those laws of organic change which shall render the same service to morphology as Grimm's law has done to the study of the

Aryan tongues. Doubtless the analogy may be pushed still farther and we may say with some confidence that just as sound principles of etymology were deduced by tracing the changes of words step by step back from their modern forms to their ancient origins, so the existing animal forms must be traced back, noting all the gradations, to their far distant ancestors.

The great problems of morphology have been attacked by the aid of many distinct methods, each of which has its own drawbacks and its own particular advantages. Most of them suffer from the fact that they deal only with the present order of things and thus somewhat resemble the attempt to work out the derivations of languages which have no literature or only one that is so falsified and vitiated with forgeries that the order of succession is hopelessly lost. Final results can be hoped for only by a combination of all the methods, but such combination becomes more and more difficult because of the continual accumulation of immense numbers of facts, which requires strenuous labor to keep track of, even in a single department, not to mention the theoretical taint now attaching to nearly all morphological work and which causes hesitation in fully accepting the results of any inquiry. While the immensity of the task and its manifold variety necessitate specialization of research, too narrow a specialization of knowledge is a great evil and many an investigator might save himself from serious blunders, could he but learn the results gathered by fellow workers in somewhat different fields, results which have a most important bearing upon his own.

The principal methods of morphological inquiry have been comparative anatomy, embryology and palæontology, each with the limitations and advantages peculiar to itself. Not long ago, and to some extent still, embryology was looked upon as the final arbiter in all morphological questions, but the many difficulties in the way of applying the method to particular cases and the lack of any generally accepted canons of interpretation, have led to a reaction against embryological deductions, which perhaps undervalues this method as much as it was overestimated before.

Comparative anatomy suffers from the drawback that it cannot with certainty distinguish between resemblances due to genetic affinity, on the one hand, and those which are the results of convergence or parallelism, on the other, and it possesses no trustworthy criterion, by which it can test the taxonomic significance of structural characters. Besides, this method deals only with the recent animal world, a mere disjointed fragment of what has existed in times past. It is a language without a literature to register its changes.

Palæontology has its manifest deficiencies, as well as the other methods. Of these the most important is the well known and oft commented on imperfection of the geological record and another almost equally grave consists in the fact that, except under the rarest and most favorable circumstances, only the hard parts of an animal can be preserved in the fossil state and very important structures and organs can only be inferred from scanty data. In spite of these defects, the method has certain preëminent advantages. It does not profess and never can hope to yield us the whole history of life, but certain chapters in that history have been preserved with wonderful completeness and fullness of detail. While it is still impossible to avoid all preconceived ideas in the construction of phylogenies, there are, nevertheless, some series so complete, so intimately connected, that no one doubts the real relationships of their members to one another. These series, which are fortunately in widely separated phyla (mammals, cephalopods, brachiopods, crustacea and echinoderms are the most important in this connection) offer important evidence as to the actual steps of organic descent, and consequently as to the mode in which organic changes may be brought about. They represent the literature by means of which the changes of words have been traced from the ancient languages to the modern and which have enabled etymologists to deduce the laws of change and establish criteria by which derivations can be thoroughly tested.

Of late a new method of attacking the problems of morphology has been suggested by Bateson,* viz: the study of variation. This book has hardly received from palæontologists the recognition which it deserves; it is especially admirable for its freedom from dogmatism, the perfect candor of spirit with which it has been written and the keen criticism which brushes away many of the obscuring cobwebs that have gathered about zoological inquiry. The author recognizes the deadlock which seems to threaten morphology as carried out along its present lines and exposes the hollowness of the prevalent modes of "explaining" the facts of evolution by means of the glib use of elaborate phrases, which too often serve as a mere disguise for ignorance. These elastic terms may be so manipulated as to "explain" anything and everything, but in reality they explain nothing at all and only darken counsel by diverting attention from the actual difficulty. Out of this labyrinth of speculation and uncorrelated facts, Bateson believes that the best chance of escape lies in the study of

* Materials for the Study of Variation, treated with especial Regard to Discontinuity in the Origin of Species. London and New York, Macmillan & Co. 1894.

variation. "Those who reject the particular inferences, positive or negative, here drawn from that study, must not in haste reject the method, for that is right beyond all question." (p. xi.) "It is submitted that the study of variation gives us a chance and perhaps the only one, of arriving at this knowledge" [i. e. of the facts of evolution] (p. 13). "In introducing the method of the study of variation, I have said that it alone can supply a solid foundation for inquiry into the manner by which one species arises from another. The facts of variation must therefore be the test of phylogenetic possibility. Looking at organs instead of species, we shall now see that the facts of variation must also be the test of the way in which organ arises from organ and that thus variation must be the test of homology" (p. 30).

Bateson's entire argument is founded upon the assumption that individual variations form the material, out of which new species are constructed, an assumption which has almost passed into an axiom. "It is upon the fact of the existence of this phenomenon of variation that all inductive theories of evolution have been based" (p. 3). He has entirely omitted any consideration of the palæontological facts and appears not to be familiar with the literature of this subject. Had he been so, he would hardly have said: "It is really a strange thing that so much enterprise and research should have been given to the task of reconstructing particular pedigrees—a work in which at best the facts must be eked out largely with speculation—while no one has ever seriously tried to determine the general characters of such a series" (p. 14). Palæontologists have repeatedly endeavored to do this and it is much to be regretted that workers in other lines of morphology should pay so little attention to their results. Progress cannot be hoped for except by combining all classes of facts. The object of this paper is to compare Bateson's methods and results with those drawn from a study of fossil mammals, with special reference, of course, to those phyla which may be regarded as fairly well established. The outcome of such a comparison is an unpleasant alternative: either the study of variation offers us little hope of learning the real facts of evolution and the assumption upon which it is founded is radically faulty, or all the palæontological phyla have been erroneously arranged and must be thoroughly reconstructed, though no reconstruction at present possible would bring about any greater harmony of results. Such rearrangement is necessarily limited by the geological succession.

Bateson's definition of variation may be accepted as an exact statement of the significance to be attached to the term. "To this phenomenon, namely, the occurrence of differences

between the structure, the instincts or other elements which compose the mechanism of the offspring and those which were proper to the parent, the name variation has been given" (p. 3). In most species, while no two individuals are exactly alike, the greater part of them resemble one another very closely and thus make a normal from which few individuals depart very widely; the magnitude of such departures is in general proportionate to their infrequency. The phylogeny of a species is given by the shifting of this normal, or center of "organic stability" (to use Galton's phrase) *not* in all the variations of the successive species, which are more or less different in every generation. While the species remains stable, the variations tend to balance and correct one another through the operation of what Galton has called the regression to mediocrity, while a material change in the position of the normal, either sudden or gradual, results in the formation of a new species. The fact that the variations of the species may be so arranged as to form a complete transition between them, does not, as Bateson very pertinently shows, prove that the actual change was along the lines indicated by such a selected series of variations.

Turning now to the examination of Bateson's results, we find that the one upon which he lays most stress is the proof of discontinuity in variation. "The existence of discontinuity in variation is therefore a final proof that the accepted hypothesis is inadequate . . . . For if distinct and 'perfect' varieties may come into existence discontinuously, may not the discontinuity of species have had a similar origin? If we accept the postulate of common descent, this expectation is hard to resist. In accepting that postulate it was admitted that the definiteness and discontinuity of species depends upon the greater permanence or stability of certain terms in the series of descent. The evidence of variation . . . . . suggests in brief that the discontinuity of species results from the discontinuity of variation. This suggestion is in a word the one clear and positive indication born on the face of the facts. Though as yet it is but an indication, there is scarcely a problem in the comparison of structures where it may not be applied with profit." (p. 568).

The mammalian phyla do not support this view of discontinuity in the origin of species and genera. Remembering that the significant fact in the history of a group is not so much the character of its variations at any one stage as the gradually shifting positions successively occupied by the normal or center of stability, we find that any mammalian series at all complete, such as that of the horses, is remarkably continuous and that the progress of discovery is steadily filling

up what few gaps still remain. So closely do the successive stages follow upon one another, that it is sometimes extremely difficult to arrange them all in order and to distinguish clearly those members which belong in the main line of descent and those which represent incipient side-branches. Some phylogenies actually suffer from an embarrassment of riches. In the case of intimately connected formations, which follow upon one another with little or no break, such as the White River and John Day, or in the successive strata of an uninterrupted horizon, the progress of this normal is by almost imperceptible gradations, though the variations may and often do describe a wide circle around the normal at every stage of its descent, circles which have a striking tendency to repeat themselves in the successive stages. Disregarding side branches, such as *Anchitherium*, *Hipparion* (probably) and *Hippidium*, the equine series from the Eocene on consists of eight well defined genera, and though they are well defined in one sense, the gradual change from one into the other is almost without a break. The same is true of other phyla and in general the continuous nature of a phylum, as at present known, is apt to be proportional to the relative abundance of its representatives in the strata, which is as much as to say that well known series are continuous, while apparently discontinuous series are those which are imperfectly known. Indeed, the most striking fact about any well established phylum is the directness of its advance towards its final goal, however greatly or in however many different directions it may be varying at any or all stages of its history. This does not imply that a line may not give off side branches or may not bifurcate into two lines. But while we represent such ramifications by branching lines in our genealogical trees, it must not be forgotten that each of the branches, considered only with reference to itself, forms a direct line from ancestor to descendant.

Continuity in the advance of a series of animals is not peculiar to mammals; it recurs wherever the phyla can be worked out in detail. Thus Waagen says of Ammonites: "Sehr häufig zeigt sich nämlich bei den hierher gehörigen Ammoniten, dass mehrere auf einander folgende Schichten Formen ein und desselben Bildungstypus beherbergen, welche einander äusserst nahe stehen, die mit einander näher verwandt sind, als mit allen übrigen in den gleichen Schichten liegenden Arten, bei denen endlich nur bei sehr eingehenden Studien und sehr reichhaltigem Materiale endlich Unterschiede gefunden werden können, die sich in allen Fällen als stichhaltig erweisen. Solche Bildungstypen kann man oft durch eine grosse Zahl von Schichten hindurch verfolgen, aber in jeder Schicht zeigen die Individuen eine von den vorhergehenden und nachfolgenden

etwas abweichende Gestalt; das Ganze bildet eine zusammenhängende Reihe, die man am besten mit dem technischen Ausdrucke 'Formenreihe' belegen könnte." "Gewöhnlich sind die Unterschiede zwischen den einzelnen Mutationen um so minutiöser, je inniger verbunden die Schichten erscheinen, denen die Stücke entstammen."*

That variations are often discontinuous may be freely granted, without at the same time admitting such discontinuity to be a frequent characteristic of phyletic history. On the other hand, such a case as the transformation of the Texan species of *Saturnia*, when taken to Switzerland and reared on a new food-plant, into what might almost be called a new species, is a remarkable instance of the possibilities of abrupt change (Moritz Wagner, Die Entsteh. d. Arten, etc., Basel, 1889, pp. 307–310). Though the results of palæontology are too few to prove a negative, they strongly suggest the advisability of caution and reserve upon this point.

II. The second class of Bateson's results refers particularly to homologies. As these require somewhat extended consideration it will be most convenient to allow the author to state his case as fully as the limits of these brief notes will admit. "Thus to compare the members of a series [i. e. of repeated parts or organs] containing different members it is first assumed that the series consisted ancestrally of some maximum number, from which the formula characteristic of each descendant has been derived by successive diminutions. . . . . If it is true that each member of a series has in every form an individual and proper history, it follows that if we had before us the whole line of ancestors from which the form has sprung, we should then be able to see the history of each member in the body of each of its progenitors. In such a series the rise of an individual member and the decline of another should then be manifested" (pp. 31–32). Our author then proceeds to give large numbers of cases of variations in the number and character of the vertebræ and ribs, of the teeth, of the digits, and of the carpal and tarsal elements, which he believes to confirm the view of homology formulated above. The consideration of the vertebræ and ribs will be omitted here, because the palæontological evidence as to these organs is too scanty to be available, the number of altogether complete skeletons of fossil mammals which have been found, being of course very limited.

With regard to the teeth Bateson arrives at the following conclusions: (1) Domestication is not a factor in causing frequency of variation. (2) Dental variation may be symmetrical

* W. Waagen, Die Formenreihe des Ammonites subradiatus. Benecke's Geognost.-Palæontol. Beiträge Bd. II, pp. 179–256.

on the two sides of the mouth or with regard to the upper and lower jaws, but much more frequently is not so. (3) In many cases of increased number of teeth it appears that two teeth in the variant represent one in the normal and have arisen by the division of a single tooth-germ. (4) Most cases of numerical variation of teeth affect those standing at the beginning or ends of series. (5) New teeth arising in particular places have a more or less constant size and the size of such a tooth is often related to that of adjacent teeth and to the general curves of the series. (6) The teeth next to which new ones arise may vary in correlation with them. (7) A few cases show the members of a series to have been remodelled so that their supposed individuality has been lost. (8) Individuality should not be attributed to members of a series which has normally a definite number of members.

The results of the examination of varying digits may be summed up as follows: (1) The frequency of digital variation differs greatly in different animals. (2) In particular animals the variation tends to approximate particular types. (3) The variation may affect both sides similarly and simultaneously, though usually differing in degree on the two sides. Similar and simultaneous variations frequently occur in both manus and pes, though rarely identical in the two. (4) When a digit is added, the adjacent one may be modified in accordance with its new place in the series. (5) There is no sharp distinction between the duplicity of a given digit and other modes of addition to the series. (6) Digital variation is often markedly discontinuous. (7) No distinction can well be drawn between those variations which resemble the normal condition of other animals and those which do not.

"The attribution of strict individuality to each member of a series of repeated parts leads to absurdity and in variation such individuality may be set aside, even in a series of differentiated members. It appears that the number of the series may be increased in several ways not absolutely distinct, that a single member of the series may be represented by two members, that a terminal member may be added to the series, and also that the number of the members may change, no member precisely corresponding in the new total to any one member of the old series; in short, that with numerical change resulting from meristic variation [i. e. variation in the number of repeated parts] there may be a redistribution of differentiation." (p. 570).

These are most important and far reaching conclusions, which, if established, will profoundly modify many of the current conceptions of morphology. That they follow fairly from the evidence of variation presented in the body of the

work must be admitted, provided only that the fundamental assumption upon which the entire argument depends be granted, namely, that individual variations form the starting points of new species.

Let us now proceed to examine these results in the light of palæontology, confining our attention principally to those mammalian phyla whose history is known with some degree of completeness. Little confidence may, perhaps, be felt in these phyla by those who have not especially studied them and it may be imagined that more complete knowledge will require them to be altogether remodelled. Granting all that may be said with regard to the incompleteness of the record and the danger of building ambitious hypotheses upon such narrow foundations, nevertheless we cannot avoid seeing that several facts stand out clearly and are not in the least likely to be overthrown by new evidence. A certain limit of error in the construction of phylogenies is established by the order of geological succession. So far as the lacustrine Tertiary formations of North America are concerned, the evidence derived from the actual superposition of strata fails at only three points. As yet, no contact between the Uinta and White River, White River and John Day, or between the Deep River and the Loup Fork, has been observed, and yet in all these cases indirect evidence of almost equal cogency is available and there can be very little doubt that this order of geological succession is known with sufficient accuracy. This order being admitted, there is no room for any very radical reconstruction of the phylogenies. By no possibility can *Hyracotherium* be derived from *Equus* or *Poëbrotherium* from *Auchenia*, and whatever changes in the details may be necessitated by further knowledge, such changes are not likely to revolutionize the inferences to be drawn from a study of the phyla as now constructed. Few observers, if any, would now uphold the arrangement of the equine phylum proposed by Kowalevsky, namely, *Palæotherium—Anchitherium—Hipparion—Equus*, and yet it is surprising to see how the general character of this series and the inferences as to the modes of evolution drawn from it agree with those deduced from a study of the equine phylum as we now have it. Kowalevsky's mistake merely consisted in putting certain representatives of side branches into the main line of descent and that a similar error has been made in phylogenies now accepted is not at all improbable. But the correction of such errors will change the general result but little and we may appeal with considerable confidence to the inference from these phylogenies.

The study of the mammalian phyla brings out a number of facts which I have elsewhere* endeavored to set forth and which do not at all agree with those inferences drawn by Bateson from an examination of the individual variations of existing mammals. One is greatly tempted to conclude that the two orders of facts are not, so to speak, commensurable. Thus, Bateson hesitates to attribute to any one the explicit maintenance of the thesis already quoted, namely, "that a series [of parts or organs] consisted ancestrally of some maximum number, from which the formula characteristic of each descendant has been derived by gradual diminution. Here, again, I do not doubt that many who employ this assumption would hesitate to make it in set terms, but nevertheless it is the logical basis of all such calculations." The mammalian palæontologist, however, need not hesitate to adopt this thesis, not as an assumption, but as an inference from many facts and to defend it with a great array of evidence, though always remembering that nature declines to submit to our rigid systems and in such matters will make exceptions to modes of procedure which are, on the whole, remarkably constant. Thus, there can be very little doubt that the numerous uniform teeth of the toothed whales are not primitively simple forms inherited from reptilian ancestors, but as *Zeuglodon* indicates, the dentition has been simplified and greatly increased in numbers since the differentiation of the order. It would be a fruitless task to attempt to homologize any of the teeth of *Delphinus* with those of *Zeuglodon*. Kükenthal has suggested with much probability that the numerous teeth of certain edentate genera $\left(\frac{20-25}{20-25}\right)$ have been derived from the subdivision of complex molars of the glyptodont type. These facts, so far as they go, agree with Bateson's conclusions, but then it should not be forgotten that they are very exceptional. Among heterodont placental mammals there is but one case known as to which it can be maintained with any show of probability that it has developed an additional tooth, and that is *Otocyon*. Though, for reasons which cannot be discussed here, I believe the fourth molar of *Otocyon* to have been added since the separation of the canine phylum, we cannot attribute much weight to this case until the history of the genus has been determined, for it is quite within the bounds of possibility that this form has retained the marsupial formula, not reacquired it. This is the view held by most writers on the subject. The vast

* On the Osteology of Poëbrotherium, Journal of Morphology, vol. v, pp. 1–78.
On the Osteology of Mesohippus and Leptomeryx, Ibid., pp. 301–406.
The Mammalia of the Deep River Beds, Trans. Am. Phil. Soc., vol. xvii, pp. 55–185.

majority of placental mammals have another mode of dental evolution, viz: that of reduction, the ancestor in nearly all cases having a larger number of teeth than the descendant. The early Eocene representatives of all the placental orders, except the rodents, have a uniform dental formula, $i\frac{3}{3}\ c\frac{1}{1}\ p\frac{4}{4}$, $m\frac{3}{3}\times 2 = 44$, a maximum which is never exceeded. Tracing the various phyla down from their Eocene progenitors, we find that in nearly all cases there is a gradual reduction in the number of teeth, never (with the one doubtful exception) an increase. Throughout the history of the phyla we may and often do find just such individual variations as those which Bateson has described, of abnormal increase or decrease in the number of teeth, but these in no way affect the march of successive species and genera, as indicated by the character of the normal or position of the center of stability, a march which is almost invariably in the direction of reduction, though in a few cases the original number is retained. Wortman's dictum: "there are no cases known to me in which teeth have been added. On the contrary, I am firmly of the opinion that not so much as a single tooth has even been added to the diphyodont mammalian dentition in the course of development, but that specialization has invariably gone in the other direction," is perhaps too strongly expressed, but is, on the whole, supported by the facts of palæontology.

Further than this, the individuality of the teeth is preserved in a remarkable way. One tooth may, it is true, assume the form and function of another; thus, in the oreodonts the lower canine has taken on the shape and character of the incisors, while the place of the canine is assumed by the first premolar, which has become caniniform, but as this tooth bites *behind* the upper canine, these changes do not in the least disguise the homologies. Whenever the successive members of a phylogenetic series have been recovered, there is no difficulty in determining what teeth have been lost. While reduction usually affects first the ends of the series, it does not always do so. For example, the camels have retained the first premolar, though losing the second. Throughout the whole history of the Carnivora, from the earliest dog-like forms with undiminished dental formula to the cats, in which the cheek-teeth have been greatly reduced in number ($p\frac{3}{2}$, $m\frac{1}{1}$) the individuality of the sectorials has been preserved. These teeth, when present at all, are always the fourth upper premolar and the first lower molar. This invariable homology is shown both by the steps of the reduction, all of which can be followed in many lines, and also by the relation of these teeth to the milk-dentition.

After these general statements, we may examine Bateson's conclusions with regard to the teeth somewhat more in detail. (1) The effects of domestication are of course not significant in the present connection. (2) Dental variation is more commonly unsymmetrical on the two sides of the same jaw and with reference to the upper and lower jaws. The mammalian phyla show no such lack of symmetry in dental reduction, though the upper and lower jaws do not necessarily keep pace with each other in this respect. At any stage in the phylum we may find *individuals* with such unsymmetrical dentition, but they are always rare, the normal form having almost invariably an equal number of teeth on the two sides of the mouth. The only exception to this rule, if it can fairly be called so, is to be found in the minute incisors of *Titanotherium*, which are on the point of disappearing altogether and in many individuals are present in different numbers on the two sides of the jaws, both upper and lower, but whether this asymmetry characterizes the normal of any species has not yet been determined.

(3) While in variation two teeth in the variant may represent one in the normal, being formed by the division of a single tooth-germ, no such process is of phylogenetic significance in any of the heterodont phyla, except, perhaps, the true whales. As a variation however, it is not unknown among fossils; specimens of *Amphicyon* have been described by Schlosser, in which a fourth upper molar is preserved.

(4) Numerical variations in the dentition usually affect the ends of series; this is likewise true of phylogenetic reductions in the number of teeth, but certainly does not apply to such cases of increase as occur among the Cetacea and Edentata.

(5) and (6) The conclusions as to the character of new teeth and their effects upon adjacent teeth have no application to the phylogenetic history of any of the heterodont phyla whose history is known, because, as we have already seen, such new teeth rarely, if ever, arise.

(7) In no heterodont group has any such remodelling of the dentition occurred as to obscure the individuality of the separate teeth.

(8) While the facts of variation seem to show "that individuality should not be attributed to members of a series which has normally a definite number of such members," the facts of palæontology lead to a diametrically opposite conclusion, so far as the mammals are concerned, though there is no reason to maintain that such individuality is fixed beyond possibility of change. It further seems likely that such individuality is more or less due to differentiation and is less distinctly marked

in a series of repeated parts consisting of numerous and uniform members.

In considering the structure of the extremities, we shall find the same discrepancy between the facts of variation and those of phylogenetic progress. The history of the changes in foot-structure has been worked out in several mammalian phyla with great minuteness of detail, palæontological material being particularly rich in this respect. Just as in the case of the teeth, there is a marked individuality in the elements which compose the manus and pes, an individuality which may be traced unchanged throughout vast periods of time. Indeed, we may speak with perfect propriety of the history of the astragalus or of the third digit and such history might be written, for many groups, with much accuracy and fullness. The limits of phylogenetic change in the extremities are more narrowly fixed than in the case of the teeth. Here it is demonstrably true that "the series consisted ancestrally of some maximum number, from which the formula characteristic of each descendant has been derived by successive diminution." In the Cetacea, however, the number of phalanges has, in all probability, been greatly increased beyond that found in the ancestors of the order and in the Sirenia the same process seems to be in an incipient stage, but in the terrestrial mammals there is no such increase of parts. In every such phylum the Eocene ancestors have the largest number of carpal, tarsal and digital members and from these the successive changes are always by means of the gradual diminution and loss of elements, partly through the coössification of members originally distinct, partly through the entire suppression and loss of members. These changes are, as has been said, extremely gradual and rudiments often linger a very long time after they have lost all obvious functional importance.

It may be objected that the phyletic series have been arranged on this presupposition and that a different arrangement would bring forth a different result. But this is not the case. As we have already seen, the geological order of succession is a bar to any wholesale reconstruction of the phylogenies, and I know of no case in which there is any reason to derive a four-toed genus from a three-toed genus. Didactyl forms, it is true, do occur in the Oligocene and upper Eocene, perhaps even earlier, as, for example, *Anoplotherium*, *Xiphodon*, *Diplopus*, *Elotherium*, etc., but these are all types, which for reasons independent of their foot-structure, we must regard as terminating their several series and as having died out without leaving any descendants behind them. That exceptional cases of the addition to parts to the extremities, or of the reacquisition of lost elements, may hereafter be discovered, is quite

possible, but they have not been found yet and the large number of phyla in which the law of reduction has been demonstrated is evidence that this law represents the normal method of procedure in the lines of descent of the terrestrial mammals.

In the case of the horses we may trace the gradual reduction of five digits to one and throughout all the many species which compose this great line there is no loss or even obscuring of the individuality and homology of the parts involved. Nothing is ever added, and what is lost disappears gradually, by successive well recognized steps. The phalanges first dwindle and are then suppressed, accompanied by the distal portion of the metapodials. The latter become splint-bones, which in some cases shorten to nodules and then vanish. Last of all the change affects the carpus and tarsus. So far as the horse is concerned, the only podial element which is actually suppressed is the trapezium, but in the tarsus the ankylosis of the ento- and mesocuneiforms brings about a similar diminution of the number of separate elements, though there is no actual loss of parts. While the lateral digits are thus gradually dwindling away, the median or third digit is being gradually enlarged in the same proportion, until it alone bears the entire weight of the body. This change requires a gradual readjustment of the carpal and tarsal bones to the altered character of the strains transmitted by them and all of these transformations may be followed step by step in the successive species. In the ruminants and the camels analogous changes have been produced in the same slow and steady manner, with a remarkable directness of development, individual variations apparently not affecting the result.

If the facts of digital variation, as Bateson has collected them, possessed any real phylogenetic significance, we should expect to find a close correspondence between these facts and those exhibited by the extinct forms, but nothing of the kind is observable. Indeed the utterly different character of the two classes of facts is obvious on the most superficial comparison. The cases of polydactylism which are so far from infrequent among the recent horses, demonstrate their radical difference from the processes at work in the phyletic history of that species by the fact that the carpus and tarsus in such cases are unlike those of any normal form whatever. For this reason Gegenbaur very properly refused to admit such cases as examples of "reversion."

Examining Bateson's conclusions as to digital variation more in detail, we reach the following results. (1) The frequency of digital variation differs greatly in different animals; the same is true of digital reduction. (2) In each kind of animal variation tends to produce a particular type of structure, which

also is observable in the phyletic series. Upon such correspondences, however, little stress can be laid. (3) Digital variation may be similar and simultaneous on the two sides, but usually differs in degree. In the phyletic succession, on the contrary, there is no such difference of degree. Individuals may and do vary in this way; the normal does not, but always keeps perfect symmetry and balance. In the changes of manus and pes phylogenetic change agrees with variation in showing a certain independence in the two extremities. As a rule, the pes changes first, while the manus is more conservative and lags behind, though sometimes this relation is reversed. While there can be little doubt that in the earliest mammals, or in their reptilian ancestors, the hand and foot were composed of the same number of parts arranged in the same way, yet in all the lines of descent there has been a constant divergence from this condition, though the correspondence, even in the most highly differentiated forms, is still remarkable. It may not be "necessary to suppose an independent evolution for the legs of the horse, since in the light of the facts of variation it is as easy for all to take on the new characters as for one," (Bateson, p. 27), and so far as the two hind-legs or the two fore-legs are concerned, this agrees with the facts of the evolutionary history, but, on the other hand, the fore- and hind-legs, not only of the horse, but of nearly all other mammals, pursue very independent ways of their own. Thus, the Eocene *Hyracotherium* had four digits and a rudiment in the manus while the pes had already been reduced to three. This tridactyl condition of the pes persisted till the end of the Miocene by which time the manus had attained the same degree of reduction, and in the next change, which consisted in the diminution of digits II and IV to splint bones, the two extremities keep equal pace. It is hardly necessary to observe that such independence of the manus and pes is limited and conditioned by the unity of the organism and its needs as a whole.

(4) and (5) When a digit is added, the adjacent ones may be modified according to their new place in the series. There is no sharp distinction between the duplicity of a given digit and other modes of addition to the series. These results apply only to variations; in the phylogenetic history of mammals new digits never are added, by doubling or otherwise.

(6) Digital variation is often markedly discontinuous. In the history of the terrestrial mammalian lines the facts lead to exactly the opposite conclusion. One of the most remarkable and striking features in the reduction of digits is the perfect continuity of it; it is often very slow, but gradual and steady, and its completion is often delayed for incredibly long periods

of time. Even *Hipparion* and the modern rhinoceroses retain a rudiment of the fifth metacarpal, though in neither line has this digit been functional since the Oligocene, and in other series similar facts have been observed.

(7) No trustworthy distinction can be drawn between those variations which resemble the normal condition of other species and those which do not. Bateson has been criticised for not making this distinction, but his critics have overlooked the fact that he has discussed the question and shown that such a discrimination cannot but be arbitrary. This emphasizes the difference between the facts of variation and those of phylogenetic change. No normal form of extinct mammal ever developed six digits, or three phalanges on pollex or hallux, or four cuneiforms in the tarsus. The possible modes of connection between the carpus and tarsus, on the one hand, and the metapodials, on the other, are strictly limited and when they change, they do so very gradually, allowing every step of the way to be identified. The change from what Kowalevsky has called the "inadaptive" to the "adaptive" mode of such connection may be shown to have occurred independently in all of the following artiodactyl groups; the Pecora, Tylopoda (probably) Tragulina (probably), *Suidæ* and *Oreodontidæ*, and when the phylum has been completely recovered, the change is invariably found to be very gradual. To say that natural selection has permitted only certain variations to persist, eliminating the others, is simply to beg the whole question and to cheat ourselves with an "explanation" which is only an assumption. If variations are actually the material out of which new species are made, then as Bateson well shows, the "perfection" of such variations is not the work of selection. The study of a complete phylum, each stage of which is represented by large numbers of individuals, does not favor such a view of evolution. The direct, unswerving way in which development proceeds, however slowly, is not suggestive of many trials, and failures in all directions save one.

If we may express in one sentence the most strikingly apparent difference between the facts of variation and those of phylogenetic change, it will be found in the comparatively lawless and uncontrolled character of the former. Addition of parts, coalescence or subdivision of parts, almost anything may happen, and if the facts of variation are "the test of phylogenetic possibility" then such possibilities are far wider and less rigidly controlled than we have hitherto been accustomed to believe. In phylogenetic change, on the contrary, we are impressed by the orderly advance toward the final goal, deviating very little from the direct line, though not always progressing at the same rate. By this it is not meant that the

direction of advance is something unalterably fixed; change of direction is not an infrequent phenomenon, but it is a deflection of the path, and not a zig-zag or meandering course that results. For example, the true ruminants (Pecora) were at one time provided with formidable weapons in the shape of canine tusks, which the hornless forms (e. g. *Moschus Hydropotes*, etc.) and the small-antlered muntjaks still retain, but in the other modern groups the development of another kind of weapon in the horn or antler has been accompanied by a reduction or loss of the canines. *Chalicotherium*, *Agriochœrus* and many other genera are examples of a change in the direction of development originally taken. It should be noted, however, that when such a marked deflection once occurs, it is permanent and the old course is never regained. Minor deviations, such as the peculiar elbow-joint of *Mesohippus*, may be eliminated, but even this is not usual.

How are we to reconcile such discordant evidence as to the modes of evolution as that presented by the facts of variation and that given by a study of the mammalian phyla? The difficulty perhaps lies in the fundamental assumption that individual variations are or may be incipient species. Many years ago Waagen, in the paper already quoted, made a distinction which has found little favor, but which may yet prove to be the key to the solution which all morphologists are seeking. At all events his suggestion is worthy of serious consideration. "Dabei ist nun aber wohl zu berücksichtigen, dass die einzelnen, in den betreffenden Individuen und Schichten zum Ausdrucke gelangten Erscheinungsweisen dieser Formenreihe oder Collectivart von Varietaten streng zu unterscheiden seien; sie eben sind die Arten, die einerseits zusammen die Collectivart bilden, andererseits aber selbst wieder in mehrere Varietäten zerfallen können; denn es lässt sich gewiss nicht leugnen, dass jeder dieser Formen in dem Zeitalter in welchem sie auftrat, eine von allen mit vorkommenden wohl unterschiedene Art bildete. Im Verhältnisse zu der früher vorhandenen Form des gleichen Bildungstypus mag die spätere vielleicht als Varietät erscheinen, doch ist dann das etwas ganz Verschiedenes von unseren heutigen, zoologischen oder botanischen Varietäten, welche in einer und der gleichen Zeitperiode neben einander auftreten: man muss daher streng unterscheiden zwischen räumlichen oder zeitlichen Varietäten. Um jene zu beschreiben wird der schon lange gebrauchte Name 'Varietät' hinreichen, für diese dagegen möchte ich der Kürze halber einen neuen Ausdruck "Mutation" vorschlagen. Die Art kann also an und für sich als Art, mit Rücksicht auf ihren Zusammenhang mit früheren oder späteren Formen aber als Mutation aufgefasst und betrachtet werden.

Aber auch in Bezug auf den Werth dieser beiden eben festgestellten Begriffe (Varietät und Mutation) wird sich bei näherer Betrachtung ein ganz verschiedener Werth herausstellen. Während die erstere höchst schwankend, von geringem systematischem Werth erscheint, ist letztere, wenn auch in minutiösen Merkmalen, höchst constant, stets sicher wieder zu erkennen; es ist deshalb auch auf die Mutationen ein weit grössere Gewicht zu legen, sie sind sehr bestimmt zu bezeichnen und mit grosser Consequenz festzuhalten" (pp. 185–6).

In a former paper I called attention to the fact that the history of mammalian phyla lends some support to this view, "These facts at least suggest the possibility that individual variations are not incipient species, but that the causes of transformation lie deeper and act with considerable uniformity upon large numbers of individuals. It may, perhaps, be the outcome of future investigation that, while variations are due to the union of changing hereditary tendencies, mutations are the effect of dynamical agencies acting long in a uniform way and the results controlled by natural selection. While this *may* be true, a great many facts must be gathered in its support, before it can be regarded as more than a suggestion," (Osteology of Mesohippus, loc. cit. p. 388.) At the time this passage was written, no very great importance was attached to the suggestion contained in it. At present, I am even less inclined than before to regard "Amphimixis" as the principal source of individual variations. But with regard to the validity of the distinction between variation and mutation the case stands differently; Bateson's results as compared with those of palæontology, confirm this distinction in many significant ways and emphasize strongly the difference between variation and that steady advance along certain definite lines which Waagen called mutation.

Neumayr reached the same conclusion from an examination of certain gasteropod phyla, in which every step of the successive changes could be followed "Noch andere Eigenthümlichkeiten stellen sich ein, welche die Mutationen als etwas von den Varietäten Verschiedenes bezeichnen, so in erster Linie dass in der Regel eine bestimmte Mutationsrichtung in jeder Reihe vorhanden ist, indem durch eine längere Aufeinanderfolge von Schichten hindurch immer dieselben Charaktere in demselben Sinne von einander abweichen. . . . . Ein anderer bisweilen beobachteter Fall von grosser Wichtigkeit ist der, dass die verschiedenen Glieder einer Reihe Variationen derselben Art zeigen; während also ein Theil die Merkmale gleichmässig nach einer Richtung im Laufe der Zeit mutirt, zeigen andere

Charaktere regellose Abänderungen und jede Mutation entwickelt denselben Varietätenkreis."*

All this might have been written of mammalian phyla, to which it exactly applies. Another fact of general application is that there is no necessary proportion between the variability of a group and the amount of mutation through which it passes. On the contrary, such groups may exist substantially unchanged through long periods of time, but being nevertheless extremely variable in all stages of their history. Others again may show little variation at any one stage and yet by steady advance experience complete transformation.

This mode of viewing the facts of development may be expressed by an analogy. The track of a cyclonic storm is determined by the path of the storm center, around which the winds circulate, blowing in every direction. These circulating winds would represent the variations which occur at every stage in the history of a phylum, while the course of the storm-center would represent the phylogenetic change, or mutations. Thus the cycles of variation tend to repeat themselves, though the center around which they revolve has a course of its own, dependent, not on the accumulation of these winds which happen to be blowing in the right direction, but upon factors of a much wider significance.

I am very well aware that any such view as that here suggested is opposed to many facts which appear to show that the distinction between individual variation, variety, species, genus, etc. is one only of degree and not of kind, the inconstant differences between variants being very much the same, and often greater in amount, as the more or less stable differences between species. As every one knows, it is often most difficult to determine the bounds of a natural group and systematists differ radically on such questions. But the problem is one as to factors of change. The distinction between variation and mutation does not necessarily imply an objective reality for species which does not extend to variations; there may be such a difference, but it cannot be demonstrated. What is meant is that the march of transformation is the resultant of forces both internal and external which operate in a definite manner upon a changeable organism and similarly affect large numbers of individuals. That phylogenetic advance does not consist in the selection of a few favorable variations out of a large number of haphazard changes, is irresistibly born in upon the student of real phyla. This view in no way impugns the importance of natural selection as enunciated by Darwin, though of course it is irreconcilable with the omnipotence of

* Neumayr, M. Die Stämme des Thierreiches. Bd. I, pp 60–61.

that process, such as is maintained by Weismann and his followers. The difficulties involved in this theory are very well brought out by Bateson who says that it "asks us to abrogate reason." The particular value which attaches to the facts of palæontology in this connection, is that they give us what we have every reason to believe are the actual steps of descent in the history of a genus or species. On the other hand, no one has ever observed the birth of a new species through the gradual accumulation of individual differences. How these apparently contradictory classes of evidence are to be reconciled must be determined by wider and fuller knowledge, but the way to obtain such knowledge is not to dogmatically enunciate a theory and then refuse to consider the facts which do not favor it.

Another very obvious objection to the mode of evolution here suggested lies in its apparent appeal to a mystical directing force which makes for differentiation or simplification, as the case may be, the nature of which we can hardly even hope to learn. Such mysterious forces are to be admitted only when there is absolutely no escape from them. This notion of a directing factor in evolution may be altogether illusory and yet it is difficult to shake it off. It is continually reappearing in one form or another in the writings of those who do not explicitly acknowledge it and are perhaps hardly conscious that their views imply it. The later theories of Weismann necessitate its assumption in some shape. But this force may after all be only the expression of some general law which has not yet been formulated, but if it be real, we shall not advance our science by shutting our eyes to it. We have then to endeavor to learn the facts of nature and having learned them, attempt to explain them. Many current explanations have been devised to account for assumed facts and these we can afford to neglect.

In making the suggestions contained in this paper, I am very far from desiring to propound a new theory of evolution or to dogmatically insist upon the interpretations of the facts as given. These interpretations profess to be nothing but suggestions which are not even novel, but they are derived from well established facts, which indicate in the plainest manner that we can no longer assume as a fundamental and self-evident truth, that individual variations are the material from which new species are constructed.

Geological Museum, Princeton College, Sept. 3, 1894.

Art. LII.—*An Iodometric Method for the Estimation of Telluric Acid;* by F. A. Gooch and J. Howland.

[Contributions from the Kent Chemical Laboratory of Yale College—xxxvii.]

In his valuable and extended study of possible volumetric methods for the estimation of tellurium, Brauner* has investigated the action of iodine upon an alkaline tellurite and finds that the oxidation is slow and incomplete at ordinary temperatures, while at 100° C. and in presence of a sufficient excess of iodine complete oxidation takes place, though the difficulty of estimating the excess of iodine not directly utilized in the reaction is so great as to render the method practically inapplicable to the determination of tellurium. The difficulty in effecting oxidation in alkaline solution naturally suggests the reversal of the reaction in an attempt to reduce telluric acid by the action of hydriodic acid in acid solution. Upon putting the matter to the test we find the reduction of telluric acid does take place, but we have been unable to prevent its going too far. Thus, on boiling a solution made by adding 10 cm^3 of sulphuric acid of half-strength and 3 grm. of potassium iodide in 90 cm^3 of water containing a little more than 0·1 grm. of telluric acid, the liquid darkened, deposited dark gray crystalline scales and evolved iodine which, when collected and titrated with sodium thiosulphate proved to be twenty per cent in excess of the theoretical yield assuming that the reduction should result in the production of tellurous acid. A similar experiment made in like manner excepting that the liquid was heated for fifteen minutes in a closed bottle on the water-bath instead of being subjected to boiling, with the idea that the presence of the free iodine might tend to prevent excessive reduction of the tellurium, indicated a yield of free iodine about six per cent. in excess of what it should be if tellurous acid is the sole product of reduction. In this case the titration of the free iodine was made by sodium thiosulphate in the presence of the reduced tellurous acid, but we found by independent experiment that the determination of free iodine associated with tellurous acid may be accomplished with a fair degree of accuracy provided the solution be cold and dilute, and the final reaction secured by adding the thiosulphate in slight excess and restoring the color permanently by standard iodine. Even under the most favorable conditions the tellurous acid is somewhat acted upon by sodium thiosulphate even before the free iodine is entirely bleached and the reaction in the reverse titration by standard

* Jour. Chem. Soc., 1891, pp. 58, 238.

iodine is slow in coming, though finally definite; but if the solution is hot the thiosulphate precipitates tellurium in flocky condition.

The excessive action of the iodide suggested the substitution of potassium bromide—an agent which proved of value in reducing arsenic acid as described in a recent paper from this laboratory—and, the preliminary experimentation proving to be encouraging we set about the preparation of telluric acid of definitely known strength. We weighed out accurately tellurous oxide, prepared by oxidizing presumably pure crystallized tellurium with nitric acid and igniting the product at a low red heat, dissolved the weighed oxide in a few cubic centimeters of a strong solution of potassium hydroxide, precipitated the tellurous acid by the careful addition of dilute sulphuric acid, and dissolved the precipitate in 10 cm^3 of sulphuric acid of half-strength. The solution thus obtained was treated with potassium permanganate in excess, the manganic oxide and the excess of permanganate were bleached by oxalic acid added to the hot solution, and the excess of oxalic acid was destroyed by the addition of a dilute solution of the permanganate until the faintest possible blush of color appeared. For the experiments involving the treatment of the larger amounts of tellurium every preparation was made individually in the manner described; for the experiments with smaller amounts a single preparation was made and the solution thus obtained was diluted to a half liter and portions of this standard solution were drawn from a burette as occasion required.

The equivalent of the so-called element tellurium is, as Brauner has shown* so variable with the mode of determining that constant and the preparation of material that we employed a preparation of tellurous oxide which had been tested as to its equivalent weight by Brauner's excellent method of oxidation by potassium permanganate. The results of these tests are recorded in the accompanying table. In series I the alkaline solution of the oxide was diluted to 100 cm^3 and oxidation was effected in it by standard permanganate added in excess. Sulphuric acid of half-strength was added to neutralization and then 5 cm^3 more, the liquid was heated, a solution of oxalic acid of known value was introduced to an amount slightly in excess of that needed to destroy the manganic oxide and excess of permanganate, and the surplus of oxalic acid was removed by standard permanganate. In series II the alkaline solution of the oxide was neutralized with dilute sulphuric acid and 1 cm^3 of sulphuric acid of half-strength was

* Jour. Chem. Soc., 1889, 382.

added. The oxidation was accomplished from this point exactly as in series I, the only difference being that it took place in the presence of a slight excess sulphuric acid instead of in alkaline solution. The permanganate used in both series was standardized ultimately against lead oxalate.

SERIES I.

TeO_2 taken. grm.	Oxygen required for oxidation.	Molecular weight of TeO_2 when O=16.	Mean.
0·1200	0·01202	159·7	
0·0783	0·00785	159·6	
0·0931	0·00940	158·7	158·88
0·1100	0·01119	158·	
0·0904	0·00909	159·5	
0·1065	0·01078	157·8	

SERIES II.

0·0910	0·00935	159·2	
0·0910	0·00910	159·9	
0·0911	0·00924	157·7	159.03
0·0913	0·00915	159·6	
0·0912	0·00915	159·4	
0·0914	0·00923	158·4	

The mean value of these determinations assigns to the tellurous oxide which we employed a molecular weight of about 159 and to the element tellurium an atomic weight of 127, when oxygen is 16. Brauner advocates the application of a correction made necessary by the excessive decomposition of the permanganate in the oxidation; but inasmuch as the oxidation in series I took place in alkaline solution under conditions in which we have no absolute proof that unused oxygen is liberated to any considerable degree from the permanganate, while in series II, the excess of sulphuric acid was purposely restricted to the lowest limit—the sulphuric acid having been shown in a previous paper from this laboratory* to be the chief agent in inducing the excessive decomposition of the permanganate—we incline, provisionally at least, to accept in this case the result of the analysis without correction.

In the test experiments the telluric acid, prepared in the manner described, was introduced into the apparatus for distillation with 3 grms. of potassium bromide, care being taken to insure in the 50 cm^3 or more of liquid the presence of 10 cm^3 of sulphuric acid of half strength. A current of carbon dioxide was passed through the apparatus, the solution was boiled to set free the bromine which was absorbed in

* Gooch and Danner, this Journal, xliv, 301.

potassium iodide and estimated by standard sodium thiosulphate. In handling the larger amounts of tellurium we found it desirable to make the preparation of the telluric acid in the distilling flask and to boil out the oxygen and ozone set free in the preparation before proceeding with the operation. The apparatus which we used consisted of a Voit's gas-washing flask, with sealed-in inlet tube and ground-in outlet tube, used as the distillation flask, to which was joined by a sealed joint the inlet tube of a Drexel washing bottle to the outlet tube of which was sealed a Will and Varrentrapp absorption apparatus. The washing bottle and attached bulbs contained a solution of 3 grams of potassium iodide and the former was kept cool by standing it during the distillation in a vessel of cold water. The absorption of the bromine took place almost entirely in the bottle, only traces of iodine being set free in the bulbs. The results of our experiments are recorded in the accompanying statement; Te=127, 0=16.

	Initial volume. cm^3.	Final volume. cm^3.	TeO_2 taken. grm.	TeO_2 found. grm.	Error. grm.
(1)	50	20	0·0102	0·0098	0·0004—
(2)	50	20	0·0102	0·0099	0·0003—
(3)	50	20	0·0102	0·0098	0·0004—
(4)	50	20	0·0102	0·0098	0·0004—
(5)	100	40	0·1000	0·0994	0·0006—
(6)	80	40	0·1001	0·1001	0·0000
(7)	100	20	0·1002	0·1001	0·0001—
(8)	50	20	0·1000	0·1003	0·0003+
(9)	50	25	0·5011	0·5008	0·0003—
(10)	50	25	0·5002	0·5006	0·0004+
(11)	50	25	0·5000	0·4998	0·0002—
(12)	50	20	0·5000	0·4994	0·0006—

The results of experiment agree fairly well with one another and with theory based upon the assumption that the atomic weight of the tellurium which we employed was 127. It is evident that the reducing action of the hydrobromic acid developed in the distillation is regular and that that agent is well adapted to the reduction of telluric acid to tellurous acid. The formation of tellurium tetrabromide in the concentrated acid liquid makes it impossible to tell by the color when all the bromine has been distilled, but the evidence of the experiments goes to show that the boiling of the liquid from a volume of 50 cm^3 to 25 cm^3 is sufficient, while concentration from 100 cm^3 to 20 cm^3 apparently does no harm.

Art. LIII.—*Resonance Analysis of Alternating Currents;** by M. I. Pupin, Ph.D., Columbia College.

I. *Introduction.*

The presence of upper harmonics in an alternating current wave is a fact which deserves careful consideration both on account of the purely scientific interest which is attached to it, and also on account of the technical bearing of electrical resonance upon the construction of conductors possessing appreciable distributed capacity.

That alternating current and electromotive force waves of a very great variety of forms can be produced by properly designing the pole-pieces of the field magnet, and the iron core of the armature of an alternator is a fact nearly as old as the discovery of electromagnetic induction. Fully as old is also the knowledge that a great variety of alternating current and electromotive force waves can be obtained by the induction of an intermittent current.

A careful investigation of these waves was first made more than forty years ago by Lenz† and Koosen,‡ who employed alternators with iron in the armature. They plotted these waves from the instantaneous values of current and electromotive force obtained by means of the now well-known *revolving sliding contact.* Employing the same method of investigation Joubert§ showed in 1880 that the electromotive force wave obtained from an eight pole Siemens alternator without iron in the armature is very nearly a sine wave. *The method is now known as Joubert's method of the sliding contact.* The name "*indicator diagram*" has been applied to the wave curves of current and electromotive force obtained by Joubert's method, and very properly, I think, because they do very clearly indicate the action of alternating current apparatus.

Our knowledge of the action of alternating current apparatus has been extended quite considerably by these indicator diagrams.

Although much must be said in favor of the sliding contact method of obtaining indicator diagrams, yet it must be also acknowledged that the method is a very laborious and uninteresting process of investigation. Many attempts have been made to devise some optical or some automatic method, but

* Read before the Annual Meeting of the American Institute of Electrical Engineers at Philadelphia, May 17th, 1894.

† Pogg. Ann. lxxvi. p. 494, 1849; xcii, p. 128, 1854.

‡ Ibid., lxxxvii, p. 386, 1852.

§ Comptes Rendus, vol. xci, p. 161, 1880; Ann. de l'école super. 10, p. 131, 1881.

with little success. There is another reason why a new method of studying alternating current waves seems desirable. It is this: the method of sliding contact is not sufficiently sensitive to detect small deviations from a true sine wave, and consequently it is not capable of following up the causes of these deviations, when the effects seem to be absent. For instance, the primary current of a transformer can differ very much from a true sine form when the secondary circuit is open, but when a large current is flowing through an approximately non-self-inductive secondary circuit, then the primary current can be made to differ inappreciably from a true sine wave. *The question arises now, what becomes of these causes when the secondary carries a heavy non-self-inductive load?*

This question is of deep scientific interest; it is also of considerable technical importance. For, if these causes are present at all loads and only hidden by the principal wave, then, considering that these hidden small causes can produce large effects when conditions favoring resonance arise, it is evident that they must be carefully watched and guarded against in the construction of lines possessing appreciable distributed capacity. I do not think that indicator diagrams obtained by the method of sliding contact are capable of giving a definite answer to this important question.

The method of analyzing alternating current waves by electrical resonance which I employed in the following investigation was suggested by me a year ago.* It is the object of this paper to describe this method at some length and to illustrate, by some of the more definite results so far obtained and relating principally to the causes which produce distortions in simple harmonic waves, the simplicity, sensitiveness, and reliability of the method. I shall also point out that this method of resonance analysis works quite satisfactorily even in those cases, alluded to above, where the sliding contact method would in all probability fail to detect any distortion whatever.

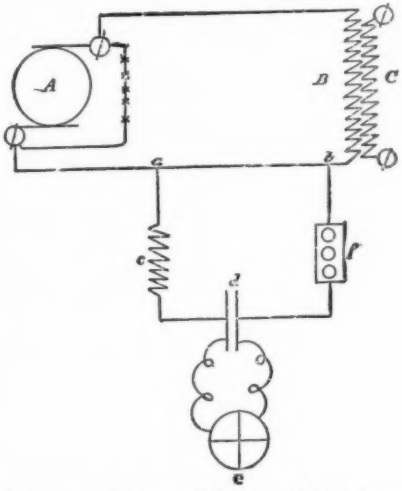
II. *Description of the Method.*

Consider the following arrangement of circuits:—The non-self-inductive resistance, *ab*, fig. 1[a], is inserted in the circuit of an alternator A and the primary B of a transformer. In shunt with *ab* is a circuit *acdb* consisting of an inertia coil *c* of large number of turns of copper wire of low resistance, about 10 ohms, but containing no iron, and a mica condenser *d* divided into subdivisions ranging from ·001 *M.F.* up. In shunt with the condenser *d* is an electrostatic voltmeter *e*. The self-induction of the coil *c* can be varied by throwing a larger or a smal-

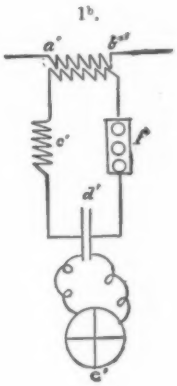
* M. I. Pupin, "Electrical Oscillations of low frequency and their Resonance," this Journal, vol. xlv, p. 429, May, 1893.

ler number of its sections into the circuit. The resistance can be varied by a rheostat *f*. Suppose now that the self-induction of *c* is kept constant, and that the capacity of the condenser *d* is gradually increased from zero up. Whenever a capacity has been reached which with the self-induction of the circuit *acdfb*

1ª.



1ᵇ.



produces resonance with one of the harmonics in the main circuit then the resonant rise of potential will produce a large deflection in the voltmeter. In this manner all the harmonics which are present in the current of the main circuit can be detected in the course of a few minutes. If the resonator circuit *acdfb* is placed in shunt with the non-self-inductive circuit *g* (this circuit is represented in fig. 1ª by a line beaded with asterisks and running from one pole of the alternator to the other) consisting of a bank of incandescent lamps then the harmonics of the impressed electromotive force can be detected in the same manner. The ratio of the amplitudes of these harmonics to that of the fundamental can also be determined by this method, *if desirable*, provided the conditions of the experiment are properly arranged. For let the current in the main circuit be

$$x = a_1 \sin pt + a_3 \sin 3pt + \ldots\ldots + a_{2\alpha+1} \sin(2\alpha+1)pt + \ldots$$

then the drop between *a* and *b* can be represented by

$$e = b_1 \sin pt + \ldots\ldots + b_{2\alpha+1} \sin(2\alpha+1)pt + \ldots$$

$$\text{where } b_{2\alpha+1} = a_{2\alpha+1} r$$

and r = ohmic resistance between *a* and *b*. Denoting now by:

L the self-induction of the resonator *acdfba*
R the resistance " " "
C the capacity " " "

then it can be easily shown* that the current in the resonator will be:

$$y=\sum_{0}^{\infty a}\frac{b_{2a+1}}{\sqrt{(2a+1)^2p^2\left\{\frac{1}{(2a+1)^2p^2C}+L\right\}^2+R^2}}\sin\left[(2a+1)pt+\varphi_{2a+1}\right]$$

If, therefore, the capacity C is adjusted in such a way that

$$\frac{1}{(2a+1)^2p^2C}-L=0$$

then the circiut will be in resonance with the harmonic of frequency $\frac{(2a+1)p}{2\pi}$; and if L is sufficiently large and R sufficiently small (two conditions which are very easily fulfilled) the current y will in general to within a small fraction of a per cent be given by

$$y=\frac{b_{2a+1}}{R}\sin(2a+1)pt$$

The amplitude of the potential difference in the condenser which is measured by the voltmeter e is then given by

$$P_{2a+1}=\frac{(2a+1)pL}{R}b_{2a+1}$$

In the same way we obtain for the fundamental frequency

$$P_1=\frac{pL}{R}b_1$$

Hence

$$\frac{P_{2a+1}}{P_1}=(2a+1)\frac{b_{2a+1}}{b_1}$$

This gives the ratio of the amplitude a_{2a+1} of the harmonic of frequency $\frac{(2a+1)p}{2\pi}$ to that of the fundamental. Let $a=2$, then,

$$\frac{1}{5}\frac{P_5}{P_1}=\frac{b_5}{b_1}=\frac{a_5}{a_1}$$

The voltmeter readings which give P_5 and P_1 magnify that ratio five times, in the case of the fifth harmonic, and it can be easily seen that a similar relation holds true for other harmonics. This is a very desirable feature of the method, considering that the amplitudes of the upper harmonics are generally small in comparison to the amplitude of the fundamental, especially when the secondary circuit of the transformer carries a load.

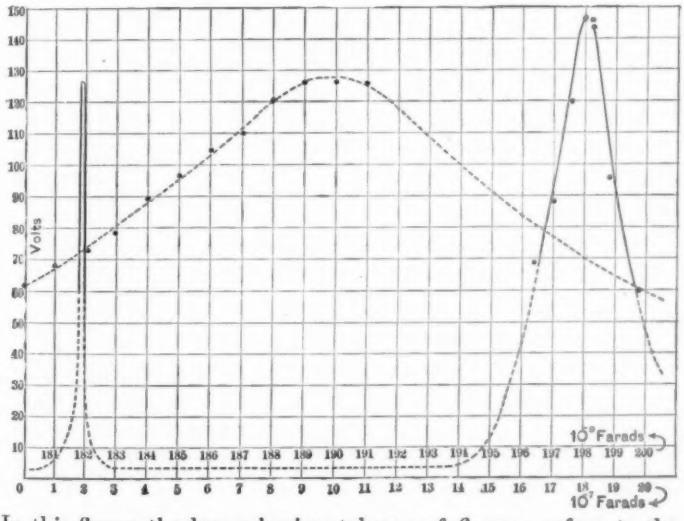
* For further information see author's paper cited above.

When quantitatively very accurate results are desired then a low resistance, say one ohm, should be used for the section *ab* and an electrometer capable of giving a large deflection for ten volts.

The principal interest, however, *in the study of the distortion of alternating current waves, is centered* not so much in the exact ratio of the amplitudes of these harmonics to the amplitude of the fundamental wave, as it is *in the causes producing these harmonics and the conditions which modify the effects of these causes.* Hence a quantitatively less accurate arrangement will do, provided that it is very sensitive, simple, and easily manageable. Such an arrangement is given, fig. 1[b].

It differs from that given in fig. 1[a] in the substitution of an air core transformer coil *a'b'* for the non-self-inductive resistance *ab*. The secondary of this coil forms a part of the resonator circuit. For every harmonic of the inducing current we shall have a harmonic electromotive force of the same frequency in the resonant circuit. By varying the capacity in the resonator and watching the voltmeter needle, we can tell by the deflection of the needle, whenever we have reached the capacity which with the self-induction of the resonator brings this circuit into resonance with one of the harmonics. A reference to fig. 2 will explain this more clearly.

2.



In this figure the lower horizontal row of figures refers to the two-peaked curve; the upper row refers to the dotted flat-peaked

curve. The vertical row denotes the voltmeter readings in volts. Consider now the two-peaked curve. It expresses the law of variation of the voltmeter readings when the capacity of the resonator circuit is varied from 0 to 2 microfarads, the self-induction being kept constant. The readings are recorded in Table I.

TABLE I.

Capacity of the condenser in microfarads.	Voltmeter readings in volts.
·18	62
·181	68
·182	73·5
·183	79
·184	89
·185	96
·186	104
·187	110
·188	120
·189	126
·190	127
·191	125
·194	99
·198	71
·202	very low
1·65	69
1·70	89
1·75	120
1·80	146
1·808	146
1·817	145
1·897	96
1·976	60

The voltmeter employed in these experiments was a Sir William Thomson's multicellular voltmeter with a range from 60 to 240 volts. The curve was obtained from a 10 H. P. Fort Wayne 8 pole alternator with a smooth core armature feeding a 5 K. W. Stanley transformer (closed magnetic circuit), the secondary circuit being open. It is seen that resonance took place at ·190 M. F. and 1·8 M. F. The capacity of the inertia coil c', fig. 1[b] and of the voltmeter as gathered from all experimental data was about ·011 M. F., so that the real capacities at which resonance took place were ·201 M. F. and 1·81 M. F., that is in a ratio to each other as $1 : 3^2$. It will be seen, however, that a very accurate knowledge of capacity is not required in the experiments described in this paper.

The frequencies detected by the two-peaked curve, which I shall call the *resonance diagram*, were therefore the funda-

mental and the 1st odd harmonic, that is the harmonic of three times the frequency of the fundamental. The resonance diagram has, of course, as many peaks as there are harmonics in the inducing current.* The dotted curve (flat-peaked) in fig. 2 was plotted on an enlarged scale from the readings taken in detecting the first harmonic represented by the sharp peak of the resonance diagram, and represents this peak spread out, so as to show how the various readings fit into a well defined and symmetrical curve such as required by theory. It also shows that a condenser of small subdivisions should be employed in detecting higher harmonics.

III. *Description of Experiments.*

The *resonance diagram* obtained by the method of fig. 1^b gives the number of harmonics which are present in the inducing current. It does not give the exact value of the amplitudes of these harmonics. It would be somewhat premature to discuss the theory of the resonance diagram obtained by this arrangement and to show how the ratio of the amplitudes of the harmonics to that of the fundamental frequency in the inducing current, that is the exact color of this current, could be calculated from the ratio of the height of the peaks in the resonance diagram. Suffice it for the present to mention only that the peaks of this diagram represent the amplitudes of the harmonics magnified about proportionally to the square of the frequency. For instance, the resonance diagram of fig. 2 tells us that the amplitude of the 1st odd harmonic in the inducing current is about one-ninth of the amplitude of the fundamental. The determination of the exact value of this ratio was not the object of the following experiments. *Their aim was to detect the presence of harmonics, to trace their origin and to study their variation with the variation of the load, and of other variable elements of the circuit on which these harmonics seem to depend.*

Preliminary Tests.

In order to form an estimate in how far the experimental data obtained by the arrangement of fig. 1^b agreed with the theory the following tests were applied:

*I have never detected an even harmonic in alternating current waves produced by ordinary commercial alternating current apparatus, and conclude, therefore, that these harmonics do not exist in such cases. For asymmetrical machines this would obviously not hold true. Alternators with slotted armatures give waves in which all the odd harmonics up to the harmonic of nine times the frequency of the fundamental can be detected. As a rule the first odd harmonic is the strongest.

a. *Study of the damping effect of the dielectric in the condenser.*

Let L = self-induction of the resonator circuit.
R = resistance of the resonator circuit.
P = amplitude of the difference of potential in the condenser when point of resonance has been reached for a given frequency.
E = amplitude of impressed electromotive force in the resonant circuit.

then according to theory

$$P = \frac{pL}{R} E.$$

Hence if R alone is varied P will vary also but in such a way that

$$P R = \text{constant}.$$

That is to say if we vary the resistance of a resonant circuit and tabulate the voltmeter deflection for every particular resistance and then plot a curve taking the resistance for abscissae and the voltmeter readings for ordinates we should, according to theory, obtain an equilateral hyperbola. Curves II and III, fig. 3, were obtained in this manner, the frequency employed was that of the 10 H. P. alternator, that is 130 p. p. s.

3.

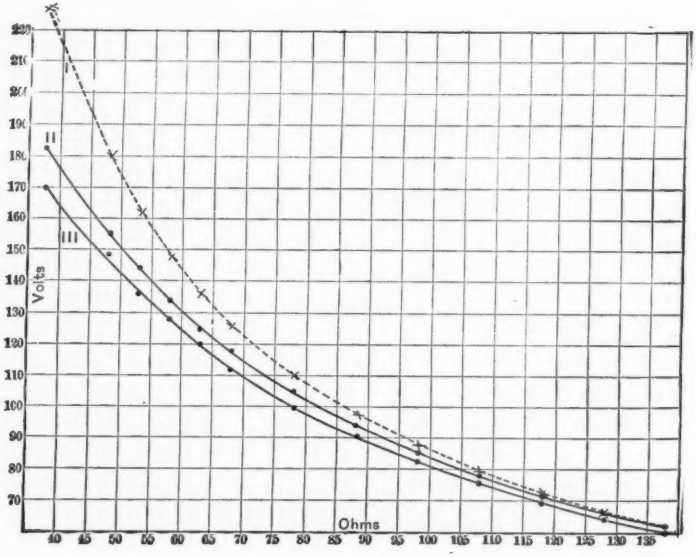


TABLE II.

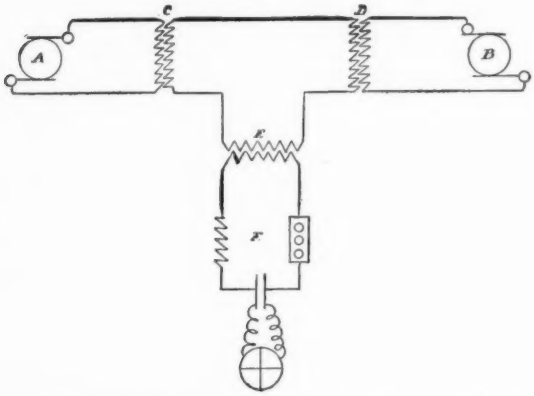
Resistance in ohms.	Voltmeter readings with a mica condenser.	Voltmeter readings with a paraffin condenser.	Theoretical value of voltmeter readings.
38	183	170	225·6
48	155	148	178·6
53	144	137	161·8
58	134	128	147·8
63	125	120	136
68	118	113	126
78	105	101	110
88	94	91	97·4
98	85	83	87·5
108	78	76	79·4
118	72·5	70	72·6
128	67	65	65·5
138	62	60	60

The experimental data from which these curves were plotted are given in Table II. Curve II was plotted from voltmeter readings obtained with a mica condenser, Curve III represents the corresponding readings obtained with a paraffin condenser and given in the third column of Table II. Curve I represents the theoretical curve, that is the curve which would have been obtained if the law of variation of the voltmeter readings with the resistance had been the same throughout as it was at low readings. On account of the damping effect due to dielectric viscosity in the condenser a deviation from the above mentioned hyperbolic relation was of course expected, but it was quite a pleasant surprise to find a perfect regularity of these deviations. These curves indicate a rapid increase in the dielectric damping with the voltage and also the superiority of mica to paraffin, especially at higher voltages. They also suggest that at low voltages and frequencies over a hundred periods per second this difference between the two substances becomes less and less marked. It was also found in a similar way that the damping effect of the magnetic viscosity of iron is small at low magnetizations, such, for instance, as would be produced by a telephonic current in a telephone receiver, and at frequencies which are well within the range of higher telephonic frequencies, say 750 periods per second.

Similar curves and similar results were obtained with higher harmonics. These experimental tests show, therefore, that the relative values of the amplitudes of the harmonics to that of the fundamental frequency are not seriously modified by the dielectric damping of the condensers, especially when one operates with moderate voltages as was the case in the following experiments.

b. Second test of the resonator indications.

4.



This test is represented graphically by diagram fig. 4. Two transformers C and D had their secondaries connected in series. The primary of the air-core transformer E formed part of their circuit. The secondary of the transformer E was a part of the resonator F. The transformer C, a Stanley 5 K.W. (closed magnetic circuit), was fed by the 10 H. P. alternator mentioned above (130 p. p. s.), the transformer D of induction coil type with a cylindrical core of fine iron wire was fed by a 1 H. P. alternator with slotted armature (278 p. p. s.) Both alternators were run simultaneously at full excitation. First, the primary circuit of the large alternator was broken, so that the current in the circuit CDE was due to the action of the small machine alone. The resonator detected a resonant rise of 240 volts at capacity ·407 M. F. and another of 150 volts at capacity ·044 M. F. These were evidently the fundamental and the first odd harmonic. Then the circuit of the small machine was broken and that of the large machine closed, so that the current in the resonator was due to the action of the large machine alone. The resonator detected a resonant rise of 220·1 volts at capacity 1·78 M. F. This corresponded to the fundamental frequency (130 p. p. s.) of the large machine. Finally both circuits were closed, so that the current in the resonator was due to the simultaneous action of the two machines. The same resonant rises of potential were detected by the resonator and at the same capacities as before, in perfect agreement with theory.

This experiment afforded another opportunity of testing the theory which underlies this resonance method of studying the

wave curves of current and electromotive force. It is this: If two or more electromotive forces of different frequencies are impressed upon the resonator circuit and their resonant rises of potential are determined for a given resistance in this circuit, then according to theory the ratio of these rises should remain the same for all other resistances within the limits within which the periodicity of the circuit is practically independent of the ohmic resistance. Accordingly, the resistance of the resonator F, fig. 4, was varied gradually from 100 to 250 ohms (the self-induction of inertia coil in the resonator circuit was about ·75 Henrys) and the resonant rises of potential produced by the fundamental frequencies of the two machines (130 and 278 p. p. s.) were carefully determined for each particular resistance. The ratio of these rises remained constant to within five per cent but the deviations were now in one direction and now in the other. They were undoubtedly due to the variation in the excitation and the speed of the small machine, both of which depended on the potential of the electric mains of the College plant which, of course, could not be kept very constant for so long an interval of time as is necessary for this experiment, which was about 15 minutes.

These preliminary experimental tests demonstrate clearly that a resonator of the type given in fig. 1^b is quite capable of detecting all the frequencies that may exist in an alternating current wave, that its indications are in good agreement with the theory as far as the fundamental frequency is concerned and that it gives a fairly approximate idea of the relative strength of the harmonics.

(To be continued.)

Art. LIV.—*On the Origin of Bitumens. A Retrospect;* by S. F. Peckham.

It is now nearly thirty years since I first directed my attention to this subject, and during this long period of time the discussion of the question has proceeded along the same lines that were quite clearly outlined in 1868.*

The chemical theories, notwithstanding the ability with which Mendeljeff treated this aspect of the question in 1877,† are at present receiving very little attention and will be passed by without further notice. In this connection may properly

* Proc. American Phil. Soc., x, 445; Rep. Geol. Sur. Cal., Geology, II; Appendix, pp. 73–90.

† Bull. Soc. Chem. de Paris, i, 501; Buer. d. Deut. Chem. Ges., 1877, p. 229; Wag. Ber., 1877, p. 1037.

be included the idea that polymerization has played any important part in the formation of natural bitumens; an idea that has lately been so ably discussed by Prof. Henry Wurtz.*

The organic origin of bitumens may therefore at present be accepted as practically undisputed. The question then recurs, are bitumens distillates or are they indigenous in the rock formations in which they are found—in other words, have they been produced *in situ?*

Twelve years ago, when preparing the essay on the Origin of Bitumens which forms a part of my report to the 10th Census of the United States, I wrote, that the derivation of bitumens had not in my judgment been uniform. That opinion took form in my own mind as a growth which was determined by many years of thought and a wide experience. This experience began with a very careful study of all the phenomena attending the occurrence of bitumen in Southern California during 1865–6, and the preparation of a report upon the same for the Geological Survey of California. I left California fully convinced that petroleum was of animal origin and formed *in situ.* Soon after I had returned to the Atlantic coast, I met Dr. J. S. Newberry in his office in New York and with Baron v. Koschkull held a long conversation upon Russian and American petroleum and incidentally upon their origin. Dr. Newberry then for the first time awakened my interest in the distillation hypothesis and based his argument upon the results that had been obtained in technical operations in Ohio in the distillation of coals and shales prior to the discovery of petroleum in Pennsylvania. The argument is all to be found in his celebrated paper on "The Rock Oils of Ohio";† but no one could resist the persuasive eloquence with which he developed the idea of that vast Sargossa sea that, gathered in the Devonian ocean, produced the Devonian shales from which the petroleum of eastern Ohio and western Pennsylvania was distilled. His speculation stopped at this point, with the idea that the oils were distillates.

Later, I was for a time in frequent intercourse with Cyrus M. Warren, whom we all remember with so much affection. He described to me in great detail the experiments he had made by saponifying menhaden oil with lime and distilling the lime soap. The amount employed was several tons, and he obtained the distillate in such quantity that a part was refined into illuminating oil, and the remainder was fractionated in Warren's apparatus. In fact, the experiment was made for technical purposes: but the results are only of

* Engineering, various dates.
† Ohio Agricultural Report, 1859, p. 605.

interest since the discovery of petroleum, for scientific purposes. This distillation yielded the hydrides of the monatomic alcohol radicals, and benzole and its homologues.

Later, Luther Atwood constructed a sort of kiln in the form of a tobacco pipe. The bowl held several tons of Cannelton cannel coal. A fire was kindled on top of the coal and the draft was urged by a jet of steam. In this apparatus distillation proceeded of necessity at the lowest possible temperature, the products of combustion expelling all the volatile matter as the combustion proceeded downward. The distillates more nearly resembled petroleum than any that had been produced by methods of distillation previously employed, apparently, *because* the distillation proceeded at the lowest possible temperature.

While this subject engrossed my thoughts I was led to consider the occurrence of bitumen in veins. I read reports on Grahamite and Albertite and Cuban asphalt and they all seemed to me to be distillates that were injected into crevices that resulted from an upheaval of the crust of the earth. At that time I had never visited the locality where Grahamite occurs, but I had seen the masses of asphaltum of vast extent that occur on the Pacific coast.* No one who has seen them doubts that they were injected into earthquake crevices. I made this question the subject of a communication to this Journal and years after, I had the pleasure of a careful inspection of the locality in which the Grahamite was obtained and talked with the men who mined it out. I saw nothing there that led me to question for an instant the relation of the Grahamite and Albertite veins to the other veins of bitumen and also to petroleum.

But distillation requires heat and the source of heat occasionally occupied my thoughts. No satisfactory conclusion was reached upon this point until one afternoon I met two of the men who helped drill Jonathan Watson's deep well. They told me their story all unconscious of what it meant to me. They said they drilled through all of the oil sands and struck the soapstone (Devonian shale) and kept on with the intention of reaching the limestone (corniferous limestone) which they believed they struck at the bottom of the well, just as, for some reason, the work was suspended and never resumed. They said the "soapstone" became harder as they went down and was redder in color, in fact had been burnt like brick. Their language at once recalled the metamorphosed shales that are so abundant in Santa Barbara (now Ventura) county, Cal., that are often as sonorous as porcelain. One day not long after, to wile away a few moments while waiting for a friend,

* This Journal, II, xlviii, p. 362; Am. Journal of Gas Lighting, xi, p. 164.

I took from a library shelf Prof. James Hall's report on The Geology of New York, and glancing at the introduction, my eye fell upon his clear description of the trend of the currents in the Silurian ocean.* First came Newberry's Sargossa Sea, which formed, when buried, one thousand feet of shale, so filled with fucoids that where the shales outcrop at Erie, Pa., it is impossible to obtain a piece of appreciable size that does not contain a seaweed. On visiting Erie and talking the matter over, I found that, at the time the distillations of coal were made in southern Ohio with which Dr. Newberry was familiar, attempts had also been made to utilize the Erie shale as a source of illuminating oil, and fifty gallons of distillate to the ton had been obtained.

The idea of metamorphism as a source of heat grew apace —heat, steam and pressure and unlimited quantities—cubic miles—of material on which to act. I had been familiar with the anthracite of Rhode Island from boyhood, and with the crystalline schists of eastern New England. I had been down in the coal mine at Portsmouth on the island of Rhode Island, and had seen the immense chambers from which the coal had been mined. All traces of bedding had disappeared, and the coal, almost graphite, had been seggregated into masses almost spherical and connected only by a thread, by which the miners traced the coal from one mass to another.

At the Palisades the molten interior mass has punctured the surface. Through eastern Pennsylvania the coal is metamorphosed into anthracite. At Saint Mary's, where the Philadelphia and Erie railroad crosses the summit of the Alleghanies, the coal is semi-anthracite. In the most easterly county of Pennsylvania in which petroleum is obtained—McKean county —the petroleum occurs at a depth of two thousand feet under a pressure estimated at four thousand pounds to the square inch and filled with paraffine, just as it ought to be if produced by metamorphism. Farther west, the petroleum becomes lighter, until, at Smith's Ferry, it is almost burning oil in its natural state. The products of distillation are all present in proper sequence along the entire line from Point Gaspè to Lookout Mt., and the porous sand bars and pebbly riffles, formed by the currents of the primeval ocean, are now filled with the oil because they afford a receptacle adequate to receive and store the vast accumulations of distillate.

Now, while I have received many very pleasant and favorable comments upon the provisional hypothesis which I ventured to propose in my Report, to the 10th Census, I have for some time had a critic who seems to partly disagree with me.†

* Nat. Hist. New York, Palcontology, iii, 45–60.

† Rep. 10th Census U. S., x, p. 59. This Journal, 1885 (?)

My friend, Prof. Orton, has in the kindest manner shown that in Ohio there is no evidence that any rocks are changed by metamorphic action. While I do not for a moment think Prof. Orton ever intended to be unjust to any one, I do think he has hardly given either Dr. Newberry or myself the position that belongs to us.*

Wise men hold any theory tentatively, no matter what it may be. When Dr. Newberry, in 1859, proposed to account for the origin of petroleum by considering it to be a distillate, very little was known concerning the occurrence of rock oil compared with our present knowledge. When I prepared my essay for the Census Report the Trenton limestone was almost an unknown factor in the production of petroleum; yet, enough was known concerning its occurrence with oil in the cavities of its fossils, to lead Dr. T. Sterry Hunt, in the same manner as other local observers; to conclude that all petroleum originated in limestone, *in situ*, and that the Trenton limestone was *par excellence* the home of petroleum.† As I have said before, when I left California in 1865, with the positiveness born of observation within a limited horizon, I was ready to declare that petroleum was formed by the decomposition of fossil animals. I first wrote the paper read at the meeting of the National Academy of Sciences at Northampton, in 1867, maintaining that narrow view; but I afterwards included vegetable remains as a precaution.‡ It has been many times demonstrated that each observer may be correct as to his own locality and wrong as to others; yet, when the consensus of opinion has been reached, the symposium may have presented a many-sided question from many points of observation, and truth may be discerned in the midst.

I therefore repeat, that in my judgment the derivation of petroleum has not been uniform. Let it be admitted that the earliest horizon of petroleum is the Trenton Limestone, in which petroleum is indigenous, having been formed by some peculiar sort of decomposition from animal remains at the bottom of a deep, excessively hot, and saline sea. If any one can prove that the Trenton Limestone extends beneath eastern Ohio and western Pennsylvania, lying beneath the Devonian shales, I am not prepared to dispute the assertion that the different varieties of petroleum found in that region have been fractionally distilled from the content of the limestone rocks, buried from three to four thousand feet beneath the present surface. I am confident that the heat required was to be found in the gradual dying out of the heated area which involved the Appalachian System. Accompanying this distillation, and a

* Rep. Geol. Surv. Ohio, vi, p. 60–83.
† This Journal, II, xxxv, p. 157–168; xlvi, p. 361. ‡ See note 1.

consequence of it, was an immense accumulation of gas. Some of it escaped into the air during the upheaval that produced the White Oak anticlinal of West Virginia, including the Grahamite vein near by, and some of it remained enclosed. There is little reason to doubt, that whether or no the oil found north of the Ohio and Kanawha rivers is distilled from the Trenton Limestones, the oils of eastern Kentucky have been. Therefore, admitting that Prof. Orton is correct in his conclusions that the sporangites of the shales are a source of oil, and that the Trenton Limestone still contains petroleum *in situ*, I still maintain my thesis, that the metamorphic action to which the Appalachian system has been subjected furnished the heat that has fractionally distilled all of the different varieties of Appalachian petroleum, either from their original source in the Trenton Limestone or through the decomposition of the shales. And further, that any consideration of the chemical constitution of these oils must show, that the oils found east of Central Ohio and north of the Ohio and Kanawha rivers are nearly pure paraffines, and that the western Ohio and Indiana oils—the Trenton Limestone oils—are mainly paraffines containing sulphur compounds from which the other oils are free; and still further, that the oils of southeastern Kentucky and Tennessee partake of the characteristics of both the Appalachian and Trenton oils.

I have lately returned to the region in Ventura County, Cal., that I visited in 1865, and that is so rich in all the phenomena that attends the occurrence of bitumen. During the years that have intervened between these visits, large quantities of petroleum have been brought to the surface, much of which has been burned as fuel. The refining of these oils still presents many interesting and involved problems. Of chief scientific importance is the discovery, lately made, and which I discussed in the paper that I read at the Mid-Winter Fair Congress of Chemists, of the manner in which the nitrogen content of these oils is chemically combined with the hydrocarbons.*

While this subject is of great interest to chemistry and technology, it is of supreme interest as related to the study of the origin of bitumens, and it is with some satisfaction that I note the sign boards that I have set up along the path that led to this discovery. While Warren produced the paraffines from menhaden oil soap, Dippel had a hundred years before produced pyridin by the destructive distillation of the gelatine of bones and the tissue of highly organized animals; but nowhere in nature had Dippel's oil been discovered until within the last two years it has been extracted from all the different varieties of bitumen found in Ventura county and its vicinity and from their distillates. These bitumens occur

*This Journal, Oct., 1894

in vast quantities in rocks that are filled with the remains of the marine mammalia that swarmed in the Miocene Ocean.*

Shall we apply Hunt's theory, and regard these wonderful compounds that appear nowhere else in nature as the product of some sort of ill-defined, inexplicable decomposition to which the animal remains have been subjected; or shall we believe that the metamorphic action which has converted into gneiss the sandstones and shales that flank the low mountains of that region, which gradually passes by insensible degrees into the granite core of the same hills in which the bitumens occur, has distilled the mixture of benzoles and Dippel's oil of which they apparently consist? To my mind the latter hypothesis alone conforms to the experience of technology and the demands of a logical scientific enquiry.

I do not wish to enter here upon any intricate inquiry concerning the nature and origin of metamorphism. I would, however, note by the way, that in addition to that unknown quantity, the internal heat of the earth, the spontaneous generation of heat by chemical action within the influence of the atmosphere has played an important part in the metamorphosis of shales in the region in which the bitumens of Ventura county occur. Locally such action has often been very recent, while, at the same time, the major portion of it belongs to a very remote past. I do not consider it necessary to represent in terms of Fahrenheit's thermometer the temperature at which any particular sample of petroleum was produced, nor do I intend to produce the coke that resulted from the distillation. The coke, if there is any, is where the distillation left it. It is not possible to furnish the details of a natural process the conditions of which are unknown to us. Nor can we reason from the processes of technology, bounded as they are by time and space, to the infinity of nature, which it is impossible to imitate. In order to establish the hypothesis that anthracite was once bituminous coal, it is not required that the distilled gases, and possibly petroleum, shall be produced, nor that it be stated where they now are. Let it be constantly borne in mind that time has always entered into the operations of nature, in sufficient quantity, and that heat, steam and pressure have invaded immense areas of the crust of the earth through vast cycles of the infinite past and the vision becomes clear:

"For a thousand years in thy sight are but as yesterday when it is past, and a watch in the night"

University of Michigan, Ann Arbor, Michigan, July 12th, 1894.

* In the course of an afternoon ramble in one of the small cañons of the mountains on the south side of the Santa Clara Valley near Bardsdale, Ventura Co., Cal., portions of three whales' vertebræ were found, one of them quite perfect. Also in one mass of rock, three vertebræ were found in contact that appeared to be those of a saurian. Besides these were innumerable fragments of ribs and other bones, in a stratum about 18 in. in thickness.

Art. LV.—*A Simple Chronograph Pendulum;* by C. Barus.

To give time to the chronograph, a break-circuit chronometer or a seconds clock is usually employed. Frequently, however, one is not in possession of this elaborate apparatus, or it is not expedient to make use of it. The following simple mechanism by which an ordinary seconds pendulum is both kept in motion and made to record its oscillations on one or more chronographs sharply, may therefore merit description.

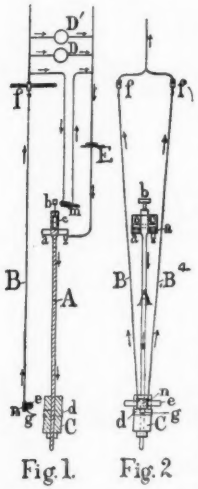


Fig. 1.

Fig. 2.

Figure 1 gives a sectional elevation of the device, figure 2 a side view with electric parts omitted. *A* is the seconds pendulum consisting of a light wooden rod (*A*) joining the heavy metallic bob *C* and the knife edge *c*. The latter is supported by the metallic plates *a*, and these and the bob are electrically connected. The top of the pendulum is surmounted by a soft iron armature *b*, actuated by the electromagnet *m* in the way presently to be described.

B is a longer and very much lighter pendulum, consisting of a flat bob *n* joined (bifilarly) by two very thin metallic wires (*B*, *B*) to the adjustable suspension *f*, *f*. The bob *n* is prevented from oscillating toward the right by a rigid post *e*. A ribbon of platinum *d* is wrapped around the bob *C* and a quadrantal spring *g* of thin platinum foil projects from the bob *n* for the purpose of securing reliable electric contacts. The bob *n* consists of two small square parallel plates, between which the wires *B B* and the platinum strip *g* are clutched by a single central screw.

Current is obtained from batteries at *E* (two or more Leclanché cells) and circulates when the circuit is closed as shown by the arrows. *D*, *D'*, etc., are the chronographs to which time is to be given, these and the electromagnet *m* being joined in multiple arc.

The mode of action is easily intelligible. Whenever the circuit is closed by the bobs at *g d* a current passes through *m*, *D*, *D'*, etc.

Suppose the post *e* and the strip *g* to be removed, and the bobs *n* and *C* to be in inelastic contact. Then on moving *A* to the left, this pendulum will push the pendulum *B* in front

of it because the *time of oscillation of n is greater*. There will therefore be a magnetic pull in *m* throughout this motion. Conversely when both pendulums having reached their extreme elongation begin to return to the right, the bob *C* will run away from the bob *n*, and hence there will be no magnetic pull in *m*. Since the bob *n* is very light as compared with *C*, the result of these unilateral impulses applied at the proper time at *b* is to keep up the motion of the pendulum *A*.

In practice however the bobs *n* and *C* are never inelastic; neither is it at all desirable to keep up the contact for so long a time. In other words the record made at *D* would be irregular from interferences and the batteries would soon run down. Therefore the bob *n* is hung so that it would meet *C* when the latter is virtually at the end of its elongation, and a stop *e* prevents *n* from swinging further towards the right. Furthermore the spring *g* causes *n* to be hurled off ballistically and the contact is now quite momentary. Indeed *n* is made flat and in falling against *e* there is *no rebound*. I have found it advantageous to surround *e* with an end of rubber hose, and also to surround the ends of the armature *b* in the same manner to prevent them from sticking to the electromagnet. Again the pendulum *B* is made to lean slightly against the stop *e*, a slide at *f* being provided for this purpose.

The records show that the time of contact does not reach 0·1 sec., and this is the result which I wish to communicate. It is desirable to use electromagnets of high resistance to prevent exhausting the battery, though I have run the pendulum for days continuously with such means as I possessed. The apparatus can easily be made symmetrical by placing a second electromagnet similar to *m*, on the other side of *b*. This would furnish another set of records differing by about half a second from the first set.

Washington, D. C.

ART. LVI.—*(1) On some new Methods of obtaining Platinochlorides. (2) Probable Existence of a Platinum Subchloride;* by M. CAREY LEA.

THE methods now in use for obtaining potassium platinochloride are: 1. Heating platinic chloride to 250–300° C. and treating with potassium chloride. 2. Passing sulphurous acid through a boiling solution of platinic chloride and treating with potassium chloride. To these older methods Thomsen has added: 3, treatment of potassium platinichloride with cuprous chloride.

All these have objections—With (1) it is not easy to obtain a uniform conversion. (2) requires to be very closely watched to catch the exact moment at which the change is complete. (3) is liable to a vexatious reverse action by which platinous salt is reconverted into platinic salt at the expense of the cupric chloride present. Thomsen mentions this danger as occurring in hot solutions. It probably depends however more on concentration than on temperature. The larger the proportion of cupric chloride present in any solution the greater the tendency to reversal. In one case a half liter of mother water containing platinous salt was set aside for spontaneous evaporation. In a few days large crystals of the red salt began to form, in a few days more instead of these increasing, there was not a crystal of the platinous salt left.

These objections led me to look for something different. I have found two methods, either of which gives good results.

First Method. Potassium Acid Sulphite.

Potassium platinic chloride is to be moderately heated with solution of the acid sulphite; convenient proportions are, platinum salt 12 grams, acid sulphite 9 grams, water 160^{cc}. The mixture can be placed over a hot water bath in a covered vessel and left to itself. The reduction takes about 10 to 12 hours and is known to be complete when the solution has a pure red color free from yellow. The cover is then removed and the liquid evaporated to the crystallizing point.

If, as may happen the red chloride and the other salts crystallize out together, it is best to redissolve them by heat in a small quantity of water saturated with potassium chloride. The red salt then crystallizes out first.

Second Method. Alkaline Hypophosphites.

By reason of its great reducing powers a very small proportion of alkaline hypophosphite is capable of converting the yellow platinum salt to the red; theoretically one part of hypophosphite should reduce nine or ten parts of platinum salt. We can hasten the operation somewhat by using an excess of hypophosphite, but then must work at a lower temperature. Both methods will be given.

In using an excess of hypophosphite it is convenient to take 10 grams of platinum salt, 2 grams or even more of potassium hypophosphite, and 600^{cc} of water. These are placed in a flask and very gently heated. The best temperature is 66° to 70° C.

There is a very easy way of obtaining this temperature and of keeping it perfectly constant for any length of time, by

taking an ordinary water stove of the kind in which a chamber is surrounded on five sides by water. Such a stove is to be furnished with a Kekulé constant level, regulated to keep the water jacket half full. If now the heat is turned on so as to keep the water gently boiling, it will be found that solutions placed *on the top* maintain a perfectly steady temperature, varying from 55° to 72° C. according to the shape of the vessel, but constant for any one shape. The lowest temperature, about 55°, is obtained with an open, flat porcelain basin. It rises gradually as the shape of the vessel tends more to check evaporation. When a liter flask has about 2 inches of solution the temperature will remain steady at about 66° and this temperature is very suitable for the treatment just described.

Even with this excess of reducing agent 10 or 12 hours will be required. The solution must not be allowed to evaporate to less than one-half its original bulk.

The completion of the operation is known by the solution showing a perfectly pure ruby red color. The slightest shade of orange indicates the presence of the yellow platinic salt. It is much safer to allow the solution to evaporate spontaneously. If evaporated by heat there is always a chance that the reduction may go too far.

There is not much to choose between these two methods. The first, with acid sulphite, is the safest, because there is no danger of carrying it too far. On the other hand in the second method the red salt separates more easily and completely in crystallizing.

On the whole the method which I prefer is to keep down the hypophosphite and use a higher temperature and longer heating. For this, a weighed quantity of platinum salt may be placed in a flask with 30^{cc} of water for each gram of the salt and a quantity of potassium hypophosphite equal in weight to $\frac{1}{3}$ of the platinum salt. The flask is to be placed in a water bath which is kept at 80° to 90° C. In consequence of the small proportion of hypophosphite the action is slow, requiring about 18 or 20 hours for complete conversion. No attention during this time is required and the advantages are that the solution becomes sufficiently concentrated to crystallize on cooling and that the very small quantity of foreign matter introduced renders it easy to obtain a pure product.

At 100° C. the reduction to red salt takes place in about fifteen minutes. This method is practicable, but requires great circumspection. If the boiling is continued a little too long, the solution suddenly turns brown; the reduction has gone too far.

If a quick reduction is desired it is better to use an acid sulphite as a reducing agent and the following method gives

satisfactory results. In a flask is placed 300^{cc} of water, 24 grams of potassium platinic chloride, 12 grams each of potassium acid sulphite and potassium chloride. Sodium acid sulphite should not be used. The introduction of sodium salts interferes with the crystallization; not indeed with the first crop of crystals, but later. These are made to boil rapidly together for twenty-five minutes, reckoned from the time when actual boiling begins. The solution is allowed to cool, filtered if necessary and placed in a large flat bottomed glass or porcelain vessel. In a day or two the red salt will commence to form large crystals. The addition of the potassium chloride causes the red salt to crystallize out first.

It has seemed worth while to give these methods in some detail because the red platinum salt is likely to find a constantly increasing use in photography not only for platinum printing but as a substitute for gold in toning. There is no doubt that platinum is a much better metal for toning silver prints than gold. Its tones are better and its action is much more reliable.

By all these methods this beautiful salt is obtained in fine ruby red prisms.

Probable Existence of a Platinum Subchloride.

If in obtaining potassium platinochloride with the aid of a hypophosphite in excess, the heat is continued after complete conversion to the red salt, the solution in a few minutes changes from red to dark brown. The substance which gives the solution this dark brown color exhibits the following properties.

It is very deliquescent and cannot be crystallized. There is no satisfactory method of separating it from the other substances in solution. An oxid of platinum appears to be precipitated by the addition of potash and this precipitate when freshly made dissolves easily in hydrochloric acid, but if it is thrown on a filter and washed, almost the whole of it runs through. This difficulty it is true can be avoided by washing with a dilute solution of potassium chloride. But the precipitate after washing is no longer soluble in hydrochloric acid, except that the acid dissolves out a little protoxid derived from the red salt, some of which is apt to escape reduction.

The brown solution exhibits the following reactions.

Hydrochloric acid has no effect.

Nitric acid decolorizes it.

Potash produces a brown precipitate soluble in an excess of the precipitant.

Ammonia a brown precipitate insoluble in an excess.

The solution itself is opaque by reason of its intense color. When largely diluted it is yellowish brown and perfectly transparent.

From the method of obtaining this substance there seem to be only two possible explanations of its nature. First, that it is metallic platinum in a state of solution; this is decisively negatived by the reactions just described. Second, that it is a chloride containing less chlorine than platinous chloride; therefore a sub-chloride. If the precipitate obtained by potash could after washing be dissolved in hydrochloric acid its constitution could easily be determined. But during the washing it seems to be converted into metallic platinum.

I have noticed that when a solution of the ruby red salt $2KCl, PtCl_2$ is spread on paper and exposed to sunlight it does not blacken but assumes a yellowish brown color; it would seem therefore that light acts upon it much in the same way as a hypophosphite, reducing it probably to a sub-chloride. If the reduction was to metallic platinum this would be shown by the production of an intense blackness.

In all this, analogy with silver salts is unmistakable. Pure silver chloride is not reduced to metal by the action of light, for after exposure it yields nothing to nitric acid. Both metals seem to form sub-chlorides, the oxids corresponding to which are very unstable.

ART. LVII.—*A Study of the Cherts of Missouri;** by EDMUND OTIS HOVEY.

THE investigations herein reported were carried forward on material collected by the officers of the State Geological Survey of Missouri and kindly furnished me by Mr. Arthur Winslow, when State Geologist, together with a long list of references to the literature on the subjects of flint and chert. The chemical analyses which will be quoted were made, unless otherwise stated, at the office of the survey, by Mr. J. D. Robertson. Thirty-eight specimens from different parts of the State, and fifty thin sections made from them, were examined.

The material falls naturally into two groups according to geological age. About half of the specimens came from the Lower Magnesian Series† (Cambrian?), and the rest from the

* Read before the Geol. Soc. Am., August, 1894.

† The Missouri strata which have long gone by the name of the "Magnesian Limestone Series" have been designated the "Ozark Series" by Prof. G. C. Broadhead (Amer. Geol., vol. viii, 33, 1891) and the term has been adopted by the Missouri Geological Survey.

Lower Carboniferous. It is aside from the purpose of the writer to discuss the geologic relations of the cherts, but the Lower Magnesian cherts present some features which separate them more or less sharply, lithologically, from the Lower Carboniferous ones. The former contain few fossils or moulds of fossils, while the latter are usually crowded with stems and plates of crinoids and other fossils, or the cavities left by their removal, though occasionally a Lower Carboniferous chert is found which shows no indication of organic remains. In both groups the color varies very much from pure white to gray, while very many of the cherts are stained brown or black by iron, and one Lower Magnesian specimen is a decided pink. The texture, likewise, is very various, some specimens being very dense, aphanitic to the naked eye, and without fossils or cavities, while others are vesicular from the solution of pebbles or are full of cavities from the removal of fossils. There is great difference, also, in the state of preservation of the chert, much of it being almost perfectly fresh except for an outer shell of decomposition, while other has suffered alteration throughout its mass, as is shown in several Lower Magnesian specimens and in the heavy beds of "tripoli" in the Lower Carboniferous at Seneca and elsewhere.

Petrography.—These cherts consist almost entirely of chalcedony or silica in the chalcedonic state, but quartz and opal are present to some extent in some of the specimens. The slides are almost colorless and featureless under the microscope in ordinary light, but in polarized light the structure of the rock is very clearly indicated and is shown to be a very fine grained mosaic, mottled by reason of variation in fineness of grain. In several of the specimens from the Lower Magnesian series, notably one from Morgan County, the material is aggregated into small spherules. These are optically negative, which proves that they are made of chalcedony. Chalcedony has a higher index of double refraction than quartz, but the polarization colors in these sections rarely rise above gray of the first order, because the grains, whether in mosaic or in concentric spherule, are too small to give the thickness required for the higher colors. For photo-micrographs showing the mosaic and concretionary or oölitic structures of chert the reader is referred to Irving and Van Hise's treatise on "The Penokee Iron-Bearing Series of Michigan and Wisconsin," 10th Ann. Rep. U. S. G. S., 1890, Pt. 1, Pl. 24, fig. 2, and Pl. 28, fig. 2. The presence of opal silica is indicated in some of the slides by apparently amorphous areas, but possibly with more definiteness in other specimens by the solubility of a portion in KOH (caustic potash), though, as will be shown, even this is not a certain criterion. Quartz occurs in

the cherts as well terminated crystals of some size more or less completely filling cavities in the chalcedony, as drusy coatings to cavities, and as well rounded grains which may or may not form the nuclei of spherules. The crystallized quartz seems to be secondary to the chalcedony in its deposition, or to form the last phase in the aggregation of the cherts, the latter appearing to be the case where seams of coarsely granular chalcedony grade into lenses of finely granular or crystallized quartz. The strictly secondary quartz lines or fills cavities in the chert without having any apparent connection with the chalcedony. The rounded grains are evidently quartz sand which has been caught within the chalcedony as it was depositing or aggregating. From the frequent occurrence of inclusions in them they are probably granitic in their origin, and this view is strengthened by the rare presence of a grain showing the multiple twinning lamellæ of microcline. Dr. C. R. Keyes* describes cherts from the Lower Carboniferous (Burlington) of the northeastern part of the State which "upon exposure to the weather quickly slacken like quicklime to a fine, intensely white powder." This would indicate that the silica in them was amorphous, but specimens of this character were not sent the writer. Scattered through all the slides there are minute irregular scales and specks of a yellowish brown to black substance which may be referred to amorphous iron oxide (limonite) though some of them are more probably grains of magnetite. A noteworthy feature of most, if not all, of the thin sections, is a "dusty" appearance as seen in ordinary light. This dust disappears to a considerable extent when the light is cut off from the upper side of the section. The phenomenon may be due to clayey matter present in the rock. In view of the discovery by Professor H. A. Nicholson† and Dr. G. J. Hinde‡ of radiolaria in chert from the Lower Silurian (Ordovician) strata of Scotland, and by Professor W. J. Sollas§ and Dr. Hinde,‖ of sponge spicules in Carboniferous chert from Ireland, very careful search was made through these Missouri cherts for indications of anything of a similar nature. Nothing whatever of this kind was found, with the possible exception of some slender cylindrical rods in a specimen from the Lower Carboniferous at Webb City in Jasper County. The rods are noticeable in cavities in the rock, and the one which was measured was 2^{mm} long by $0{\cdot}12^{mm}$ in diameter, but it seemed to have been thickened by some

* This Journal, III, xliv, 451, 1892.
† Trans. Edinb. Geol. Soc., vol. vi, pt. 1, p. 56, 1890.
‡ Ann. and Mag. Nat. Hist., VI, vol. vi, p. 40, 1890.
§ Ann. and Mag. Nat. Hist., V, vol. vii, p. 141, 1881.
‖ Geol. Mag., N. S., Dec., III, vol. iv, p. 435, 1887.

extraneous deposit. What appear to be the cross-sections of these rods have nuclei of brown matter surrounded by clear chalcedony, but sometimes several exactly similar nuclei string themselves together within a common shell of chalcedony. Between crossed nicols there is no line of demarkation between these bodies and the matrix, all is granular chalcedony. I hesitate, therefore, to refer these rods to sponge spicules. Many of the Lower Carboniferous cherts are highly fossiliferous, being more or less crowded with the remains of crinoids, brachiopods and corals. These remains, however, are calcareous in nature and form a breccia with the chalcedony of the chert as the cement, though in the cherts described by Dr. Keyes* the fossils have been silicified. A specimen from this group at Grand Falls, Newton County, shows several sections of a branching form of *Stromatopora* and this genus also occurs in chert from Sulphur Springs, Arkansas. The chalcedony occasionally shows a tendency to form concretionary granules. In the specimen just cited there are true fibrous sphærocrystals which give a black cross in polarized light. The pronounced granular or oölitic character is confined to five of the Lower Magnesian specimens in the suite under discussion. In some of these the chalcedony has formed granules without any apparent foreign nucleus, while in others rounded grains of quartz were the nuclei of deposition. Siliceous oölite was noted by G. W. Featherstonhaugh† in Wayne County, Missouri (?), Tennessee and Kentucky and reported as "silicified oölite" of Carboniferous age, but his stratigraphy is not to be depended upon. Concretionary or granuliferous chert is one of the three divisions made by A. Renard‡ in his study of the Carboniferous cherts of Belgium. A preliminary microscopical and chemical investigation of siliceous oölite was made by E. H. Barbour and J. Torrey§ on specimens sent them from Center County, Pennsylvania. Since then the rock has been reported a second time from Tennessee‖ and it appears probable from an able discussion of the Pennsylvania oölite by Dr. W. Bergt,¶ which first came to the present writer's knowledge some months after his own article** on the same subject had been published, that several rocks from widely separated parts of the world and described under other names really belong in this category. Our specimens, therefore, add five localities for

* Loc. cit.

† Geol. Rept. of an examination of the elevated country between the Missouri and the Red Rivers, 1835, pp. 54, 55.

‡ Recherches lithologiques sur les phthanites du calcaire carbonifère de Belgique. Bull. de l'Acad. Roy. des Sciences, etc., de Belgique, II, xlvi, 1878, p. 494.

§ This Journal, III, xl, Sept. 1890, p. 246.

‖ G. R. Wieland in the Mineralogist's Monthly, vol. vi, Nov. 1890, p. 2.

¶ Abhandlung d. Isis in Dresden, 15, 1892.

** Bull. Geol. Soc. Am., vol. v, p. 627.

this interesting rock: Wright County, Camden County, Morgan County, Osage River, and Taney County, in Missouri. Mr. Winslow reports that the oölitic chert of the Missouri Lower Magnesian strata is of common occurrence and that frequently the beds are more than a foot thick. One of the Lower Magnesian localities furnishes a rock which is partly oölitic in structure and two others give rocks in which rounded grains of quartz have been cemented in an abundant matrix of chalcedony, without the formation of concretionary spherules about them. In one of the latter, however, the chalcedony shows that there was some tendency within it to form shells about the quartz grains, and an occasional small spherule of chalcedony may be seen in the matrix.

Chemistry.—As was to be expected from the microscopic characteristics, the cherts, when not fossiliferous, are almost pure silica. The analyses made by the Missouri Geological Survey show a much higher percentage of alumina and iron-oxide than is present in the cherts from the same strata and the same general region which were analyzed by the U. S. Geological Survey, but this difference is probably due to the fact that the specimens analyzed by the Missouri survey were selected more to illustrate the transitions between cherts and other rocks than to exemplify pure chert. Mr. Robertson reports that in making the analyses the $Al_2O_3+Fe_2O_3$ was redissolved after the first precipitation and precipitated again to make sure that no SiO_2 was included in the amount. The percentage of soluble silica was determined in only four of these analyses: Prof. Seamon, in his report on No. 11 for the Tripoli company, says that "7·28 per cent of the silica was soluble in a 10 per cent solution of caustic soda on boiling for three hours;" the U. S. Survey reported 4·52 per cent in No. 13, 3·99 per cent in No. 14 and 3·35 per cent in No. 18, and that the determination was made in the following manner: the solution used was made up of one part solid caustic potash to three parts water, and one gram of the finely powdered chert was heated in each case with fifty cubic centimeters of the solution for one hour on the water bath. No. 14 was somewhat porous, Nos. 13 and 18 were compact, the last showing occasional cavities filled with quartz crystals.* These percentages, however, cannot be taken as the measure of the amorphous silica present in these rocks, for undoubted quartz is noticeably soluble in caustic potash, Rammelsberg† finding from 5 to 7·75 per cent of vitreous massive quartz thus soluble, and quartz crystals and quartzite tested for the Arkansas Survey‡ gave from 2·59 to 6·28 per cent soluble in this medium.

* Ark. Geol. Surv., Ann. Rep., 1890, vol. iii, Novaculites, p. 161.
† Quoted in Dana's Syst. Mineralogy, 5th ed., p. 193.
‡ Op. cit, p. 164.

ANALYSES OF MISSOURI AND OTHER SIMILAR CHERTS.

Analyses of Missouri and other similar cherts.	1. Chert, Lebanon, La Clede Co., "decomposed."	2. Decomposed chert, Olden, Howell Co.	3. Chert, 72 ft. below surface, Spgs., Newton Co.	4. Chert, Henderson Mines, Barry Co.	5. Chert, Grand Falls, Newton Co., calcareous.	6. Calcareous chert, Sulphur Springs, Ark.	7. Calcareous chert, Aurora, Lawrence Co.	8. Ditto.	9. Decomposed chert, Webb City, Jasper County.	10. Decomposed chert, Joplin, Jasper Co.	11. Decomposed chert, ("tripoli") Seneca, Newton Co.	12. Ditto.	13 Chert, unaltered, Bellville, Jasper Co.	14. Chert, altered, Bellville, Jasper Co.	15. Ditto.	16. Chert, gray, unaltered, Joplin, Jasper County.	17. Chert, blue, unaltered, Galena, Kan.	18. Chert, white, altered, Galena, Kan.	
Silica (SiO_2)	96·88	89·45	94·91	98·33	63·67	71·29	91·25	88·92	97·03	96·14	98·100	98·28	98·17	98·71	98·92	99·46	99·23	99·13	SiO_2
Alumina (Al_2O_3) } Iron oxide (Fe_2O_3) }	2·48	0·94	2·85	2·18	2·20	2·43	1·28	1·25	2·32	1·44	0·240 / 0·270	0·17 / 0·53	0·83	0·43	0·48	0·29	0·22	0·16	Al_2O_3 / Fe_2O_3
Carb. lime ($CaCO_3$)	1·11	9·44	0·75	--	33·12	26·24	6·56	10·02	1·10	2·16									$CaCO_3$
" magnesia ($MgCO_3$)	*tr.*	0	*tr.*	--	0·57	*tr.*	0·17	*tr.*	*tr.*	*tr.*									$MgCO_3$
Lime (CaO)											0·184	*tr.*	0·05	0·03	0·03	0·04	0·02	*tr.*	CaO
Magnesia (MgO)												*tr.*	0·01	0·02	0·02	*tr.*	*tr.*	0·01	MgO
Potassa (K_2O)												0·17							K_2O
Soda (Na_2O)											0·230	0·27							Na_2O
Water (Ignition)											1·160	0·50	0·78	0·50	0·42	0·34	0·50	0·20	Ign.
Organic matter											0·008								Org.
Totals	100·47	99·83	98·51	100·51	99·56	99·96	99·26	100·19	100 45	99·74	100·192	99·92	99 84	99·69	99·87	100·13	99·97	99·50	Totals.

Notes on the analyses.—Nos. 1 and 2 were from the Lower Magnesian strata; Nos. 3 to 18, from the Lower Carboniferous. Nos. 1 to 10 were made at the laboratory of the Missouri Geological Survey by Mr. J. D. Robertson; No. 11, at the Mo. School of Mines, Rolla, by Prof. W. H. Seamon, for the American Tripoli Co., of Seneca, Mo.; No. 12, by the Arkansas Geol. Survey, and it is quoted from their Annual Report for 1890, vol. iii, p. 397; Nos. 13 to 18, by the U. S. Geological Survey, and they are quoted from the same volume of the Arkansas reports, p. 161.

The amounts given above for the Missouri cherts are below these maxima and, therefore, do not necessarily indicate the presence of opal (amorphous) silica, since chalcedony is held to have the same chemical characteristics as quartz. When a large percentage of the silica is soluble in caustic potash the presence of opal silica is indicated; e. g., the Arkansas Survey chemist found 30·72 per cent of a Silurian chert, 35·56 per cent of a Tertiary chert and 88·38 per cent of a geodized coral from Tampa, Florida, to be thus soluble. These geodized corals are known to be opal and the two cherts must contain large amounts of amorphous silica. Edward T. Hardman* analyzed a series of twelve specimens of chert from the Upper Carboniferous strata of Ireland. He tested the rocks in hydrochloric acid and found traces of soluble silica in several and 1 per cent, 1 22 per cent and 1·5 per cent in three cases. All the Cretaceous chalk flints contain much opal silica and show high percentages of silica soluble in caustic potash.†

It will be seen from the table that the chemical difference between "altered" and "unaltered" chert is so very slight that they can be distinguished only by physical characteristics. The most completely altered chert is that from Seneca, which is minutely porous, breaks to pieces readily between the fingers and may be ground to an impalpable powder in an ordinary mill. There is an extensive bed 18 ft. thick of this material at this locality and it is quarried for the manufacture of filtering disks and tubes and of a high grade of polishing powder. A similar rock occurs near Seneca in the Indian Territory, at Dayton, Newton Co., Mo., and in township 4 S., 26 W. in central Arkansas.‡ The Arkansas rock disintegrates to a fine powder on exposure to the atmosphere, but the Seneca rock does not. Another item of interest in the analyses is the very low percentage of water (by ignition) which was found, especially in the pure cherts, whether altered or unaltered. This would argue against the presence of more than a very small amount of opaline silica. Mr. Hardman's analyses of the Irish Carboniferous cherts brought out the same fact regarding the presence of water.

Origin of the chert.—There has been much speculation as to the origin of flint, hornstone and chert. The Cretaceous flints of the chalk formation in England and elsewhere contain so many remains of originally siliceous animal organisms (skeletons of siliceous sponges and polycystines) that some authors claim that all their substance has come from this source,

* Sci. Trans. Roy. Dub. Soc., I, vol. i, pp. 85, 1878.
† Vid. Geology, J. Prestwich, vol. ii, pp. 321 and 322.
‡ Ark. Geol. Surv., Ann. Rep., 1890, vol. iii, p. 384.

while others contend that the silica of the flints is entirely due to chemical precipitation. Other theories lie between these extremes. A concise summary of the various theories which have been proposed to account for the flints is given by Prof. Prestwich,* who gives as his own opinion the theory that silica in the colloid or soluble state was present as a chemical precipitate in the mud of the Chalk seas and that this colloid silica, having a strong affinity for other forms of silica, gelatinous substances (like the sarcode of sponges) and other foreign bodies, aggregated about sponge spicules, replacing the sarcode as that decayed, and about the tests of echinoderms and the shells of molluscs. The irregular masses thus produced continued to grow as long as there was any colloid silica within the range of attraction.

The Corniferous hornstone partakes so largely of the characteristics of the Chalk flints that it probably had its origin in the same way.†

The Upper Carboniferous cherts of Ireland were studied by Messrs. Hull and Hardman,‡ who came to the conclusion that the chert was "essentially a pseudomorphic rock consisting of gelatinous silica replacing limestone of organic origin chiefly foraminiferal, crinoidal and coralline," the silica being a chemical precipitate from the sea water of the period. M. A. Renard§ came to a similar conclusion for the cherts of Carboniferous age in Belgium. This view has been vigorously combatted by Dr. Geo. J. Hinde,‖ who, starting from the discovery made by Prof. Sollas that there were sponge spicules in the Irish cherts, had numerous thin sections of the rocks made for himself. From the study of these Dr. Hinde decided that the cherts were wholly organic in origin and that they were due to the aggregation and disintegration of the skeletons of siliceous sponges.

Pre-Cambrian cherts have been studied by Irving and Van Hise¶ in their work on the Penokee iron-bearing series of Michigan and Wisconsin. This series belongs to the Huronian subdivision of the Algonkian system and two of its four members contain much chert. The author's conclusion regarding the origin of the chert is, "First, that the chert was mainly deposited simultaneously with the iron carbonate with which it was so closely associated; and, second, that it is probable that the chert is of organic origin, although we have no posi-

* Op. cit., pp. 320–324. Vid. also, Ark. rep't cit., pp. 177–187, where an extended summary of theories is given.

† Cf. Manual of Geology, J. D. Dana, 3d edition, 1880, p. 257.

‡ Sci. Trans. Roy. Dub. Soc., II, vol. I, pp. 71 and 85, 1878.

§ Op. cit.

‖ Geol. Mag., N. S., Dec., III, vol. iv, p. 435, 1887.

¶ Tenth Ann. Rep. U. S. G. S., p. 347.

tive proof that it is not an original chemical sediment, while it may in part be from both sources." This statement is made to apply with equal force to the other chert-bearing member of the series.*

Regarding the Lower Magnesian and Lower Carboniferous cherts from southern and southwestern Missouri, the present writer's conclusion is, that they are due to chemical precipitation, probably at the time of the deposition of the strata in which they occur or before their consolidation.

Am. Mus. Nat. Hist., N. Y. City, July, 1894.

Art. LVIII.—*The Qualitative Separation of Chromium from Iron and Aluminium;* by R. B. Riggs.†

The separation of chromium from iron and aluminium depends on the conversion of chromium compounds, by oxidation, into soluble chromates. This oxidation is generally effected, either by fusing the hydroxide or basic acetate precipitates with sodium carbonate and potassium nitrate, or by dissolving them in concentrated nitric acid to which potassium chlorate has been added. In the hands of the general student the latter is probably the more satisfactory of the two methods. It is somewhat complicated however, involving, as it does, the separation of the aluminium by means of potassium hydroxide, solution of the residue in a potassium chlorate-nitric acid solution, and a reprecipitation of the iron.

In many cases the efficiency of hydrogen peroxide, in alkaline solutions, as an oxidizing agent, is well known. Its use in converting chromium hydroxide into the chromate was a natural suggestion. A few experiments showed that, under favoring conditions, this change is easily and completely brought about. Solutions, containing the equivalent of 0·05 grm. of chromium as hydroxide, 0·3 grm. of sodium hydroxide and quantities of hydrogen peroxide (15 per cent) varying from 5 to 20$^{c.cs.}$, varying in volume from 50 to 500$^{c.cs.}$, were digested

* Ib., pp. 397 and 368.

† Since writing this note my attention has been called to an article by Clark (Jour. Chem. Soc., 1893, i, 1079), on "The use of Sodium peroxide as an Analytical agent." He makes very successful use of this reagent in decomposing ferro chromium ores, and says that it may be used for the qualitative separation of chromium from iron and manganese, in alkaline solutions, when it acts quickly even in the cold. As compared with hydrogen peroxide, sodium peroxide is the more powerful oxidant, and if the former has any advantage, it is to be found in convenience and, possibly, economy.

until oxidation seemed* complete. This required from five to fifteen minutes: the more dilute the solution the longer the operation. $5^{c.cs.}$ of the peroxide was sufficient for the oxidation.

The availability of hydrogen peroxide, as an oxidizing agent, having been determined, the influence of its presence on the iron and the effect of dilution, on the solubility of aluminium hydroxide in sodium hydroxide solutions, remained to be ascertained. Filtrates from ferric hydroxide, which had been digested in sodium hydroxide solutions containing hydrogen peroxide, gave no sign of the presence of iron except to the sulphocyanide test, showing the influence to be rather favorable than otherwise.

In determining the effect of dilution on the solubility of alumina, quantities of the hydroxide, equivalent to 0·05 grm. of the element, were severally treated with 0·5 grm. of sodium hydroxide in solutions varying in volume from 50 to $500^{c.cs.}$ In every case solution was effected within five minutes. The influence of dilution was however appreciable.

Experiments, made, to test the practical workings of the method, on mixtures of the three substances, confirmed the observations made in connection with their separate treatments, except that the presence of large quantities of iron hinders somewhat both the oxidation of the chromium and the solution of the aluminium. As to the limits of the method the impurities, of even the best of available reagents, render the detection of very small amounts of either iron or alumina uncertain. In mixtures, containing the equivalents of 0·2 m.grm. each of iron and chromium, and 0·5 m.grm. of aluminium, separations were made and the several constituents

* In a majority of the experiments, in the more concentrated solutions, the precipitates disappeared quickly and completely. Where it was necessary to continue the heating for more than ten minutes, there remained an unoxidizable residual which increased rather than diminished on further heating. In all cases prolonged digestion resulted in a precipitation of what proved to be silica. The amount of this precipitate was found to be proportionate to the quantity of peroxide added and the following experiments showed that it came from the corrosive action of an alkaline solution of the peroxide on the glass. $50^{c.cs.}$ of water, containing 1 grm. of sodium hydroxide, digested for an hour in glass, remained clear to the end. $50^{c.cs.}$ of hydrogen peroxide, similarly heated, remained clear, evaporating quietly. $50^{c.cs.}$ of the peroxide, containing 0·5 grm. of sodium hydroxide, was also similarly digested (the alkaline solution effervesces energetically on being heated). In fifteen minutes the solution had become perceptibly clouded. At the end of an hour, the effervescence having ceased, a considerable precipitate had formed. From $200^{c.cs.}$ of the peroxide, containing 2 grm. of sodium hydroxide, 4 m.grm. of silica (carrying a trace of iron) were obtained. $500^{c.cs.}$ of the peroxide, containing 5 grm. of sodium hydroxide, heated in platinum for an hour, remained perfectly clear.

The reagents used were Rosengarten's 15 per cent hydrogen peroxide, $500^{c.cs.}$ of which gave, on evaporation, a residue of 0·2908 grm. (silica 0·1067 grm., alumina 0·02 grm.,) and sodium hydroxide made from sodium.

were identified beyond a doubt. With half the above quantities the results were not so satisfactory. The color tests for both iron and chromium are sufficiently delicate to detect even smaller amounts. The sulphocyanide test will, under favorable conditions [a solution containing not over $\frac{1}{10}$ per cent of hydrochloric acid and an excess of sulphocyanide ($5^{c.cs.}$ 1 : 15)], show the presence of 0·01 m.grm. of iron in $100^{c.cs.}$ of water (depth of liquid in cylinder 14^{cm}). The yellow color of sodium chromate is unmistakable in $100^{c.cs.}$ of solution containing the equivalent of 0·2 m.grm. of the element.

Summary.—The following is an outline of the method of treatment. Given a mixture of the hydroxide or basic acetate precipitates equivalent to 0·1 grm. of each of the three elements. Digest this precipitate in $100^{c.cs.}$ * of water, to which $10^{c.cs.}$ of hydrogen peroxide and 1 grm. of sodium (or potassium) hydroxide have been added, until effervescence ceases.† Separate the iron by filtration. Acidify the filtrate slightly with acetic acid and precipitate the alumina by means of ammonia. (Where small quantities are present long continued boiling and a concentration of the solution may be necessary.) Filter off the alumina and test the filtrate for chromium. The yellow color of either the iron or alumina filtrates is characteristic. A confirmatory and more delicate test may be made by acidifying the filtrate from alumina slightly and adding a few c.cs. of hydrogen peroxide, obtaining the blue color so characteristic of chromium compounds thus treated.

Chemical Laboratory, Trinity College,
Hartford, Aug., 1894.

Art. LIX.—*On Copper Crystals in Aventurine Glass;* by Henry S. Washington.

One of the most curious products of the world-renowned glass-works at Murano near Venice is the so-called "aventurine glass" (*vetro avventurino*), which owes its name, according to the story, to its discovery by chance (*all' avventura*), some brass filings having been dropped accidentally into a pot of molten glass. This however was not the process by which it was subsequently made, though the secret of its manufacture was lost in the decline of the glass industry towards the end of

* The volume will not materially influence the results. Unnecessary dilution is however to be avoided.

† If the peroxide be not decomposed there is danger, on acidifying the filtrate containing chromium and alumina, of a reduction of the chromate and consequent precipitation.

the last century, and was only re-discovered after the recent revival of the art some thirty years ago, due chiefly to the late Dr. A. Salviati of Venice. The glass finds now quite an extensive use in the manufacture of mosaics and of brooches and other cheap jewelry.

According to Vogt* it was one of the first substances of a mineralogical nature ever examined with the microscope, since in 1807 I. G. Gahn in Fahlun called Hausmann's attention to the fact that small three- or six-sided tables were visible in it under the microscope. Wöhler† was the first to show that these tabular crystals were of metallic copper, to whose brilliant reflections and metallic luster the peculiar appearance of the glass is due. Although, to judge from a remark of Vogt's (see below), the glass has been several times examined, yet I can find but two references to it in the literature at my disposal here. Fouqué and Lévy‡ simply mention Wöhler's determination of the character of the crystals, while J. H. L. Vogt§ devotes a couple of pages to it, and gives a few figures, but goes little into details.

Through the kindness of Signor G. Boni of Rome, to whom I desire to express my thanks, I was enabled to obtain at the works of the Venezia-Murano Glass Co. at Murano specimens not only of the finished perfect product, but of a mass that was the result of an unsuccessful melting, as well as fragments of the melting pots both before and after their exposure to the intense heat of the furnaces. The results of a microscopical examination of this material seemed worthy of publication, especially since they add to our knowledge of the crystallization of copper.

The perfect glass is of a copper brown color, and transparent to translucent in thin flakes, showing on the edges a pale brown color. It is filled with innumerable small flakes and spangles of a slightly brownish yellow color and brilliant metallic luster. This is best seen apparently in sections or fracture surfaces of the glass parallel to the original surface in the melting pot, showing that the majority of the flakes lay horizontally in the molten glass. The specimens also show streaks of comparatively flakeless material.

The imperfect glass is slightly darker in color and is less transparent, showing at the thin edges a greenish color. The spangles and glistening points are almost entirely wanting and those present much smaller than in the perfect examples. A specimen taken from the surface of the mass solidified in the

* Vogt, Mineralbildung in Schmelzmassen. Kristiania, 1892, p. 238.
† Wöhler, Gött. gelehrt. Anz., v, 1842, p. 1785.
‡ Fouqué et Lévy, Synth. d. Min. et d. Roches. Paris, 1882, p. 367.
§ Op. cit., pp. 237–9.

melting pot shows a smooth uppermost layer, about 0·2^{mm} thick, of a clear dark greenish blue color, the surface of this being covered with minute rounded pits and indented six-rayed stars.

Microscopically the perfect glass may be first described. This shows under the microscope a porphyritic structure, the groundmass being composed of a perfectly clear and colorless glass basis, which in the parts where the phenocrysts are best developed is quite clear and free from microlites, in other places and in streaks being "dusty" through the presence of large numbers of the minute octahedral microlites to be described presently.

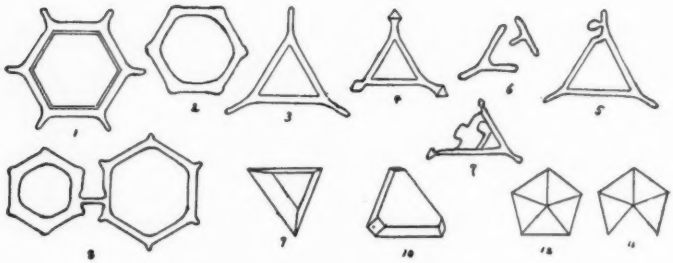
The crystallized portion of the mass consists entirely of copper and may be divided sharply into three distinct groups, large phenocrysts, small phenocrysts, and microlites, which differ greatly from each other both in the size and the habit of the individuals.

All of the large phenocrysts, which range in diameter from 0·05–0·12^{mm}, are tabular and extremely thin, the thickness scarcely exceeding 0·002^{mm} and often less than half this, being perfectly opaque notwithstanding their excessive tenuity. Most of them are hexagonal in outline, the hexagons being of almost ideal symmetry, and equilateral triangles, which occasionally show truncated angles, also occur. They show a rather yellowish copper color and in reflected light a brilliant metallic luster. The edges are perfectly sharp and straight, except those of the plates lying obliquely in the section and which are due to the making of the slide, which are rough and somewhat granular or ragged. Seen "end on" the edges show, when thick enough, a face not quite normal to the plane surface of the crystals, though they are too thin to make measurements at all exact.

The faces of these hexagons and triangles are in general perfectly smooth and plane, but a number show a peculiar appearance which is represented in figs. 1–5. In such cases we see that the central part of the face is depressed forming a very shallow hexagonal or triangular pit, symmetrical with the crystal outline, and surrounded by a salient edge about 0·004^{mm} broad, and with sloping and rounded sides. The angles of these shallow depressions are generally rounded, so much so in some cases that they assume an almost circular form (fig. 2). It may be remarked that such shallow pitted surfaces with salient edges are not rare in native copper crystals and quite common in gold.

This peculiar form, which seems to be due to a skeleton growth, is almost always accompanied by projections of greater or less length at the outer angles of the tabular crystals, as

shown in the figures, the projection being always in the direction of the bisectrix of the angle. These projections are in general rounded and show no crystal faces, but in a few cases (fig. 4) they are seen to be minute octahedra, an apex being outward and two of the dodecahedral interaxes being parallel to the plane of the tabular crystal. Some of these small octahedra show replacements of the solid angles by cubic planes, and they all gradually run into the salient edges of the main crystal.



FIGS. 1–12.—Copper Crystals in Aventurine Glass, × $\frac{200}{1}$.

In addition to these more regular forms some skeleton crystals are to be seen (figs. 5, 6, 7) which are made up of narrow ridges and show that the pit-like depressions are due to incomplete growth, and not to corrosion. A curious and quite unique form is shown in fig. 8. This is a "Siamese twin" of the hexagonal tables joined together by a narrow ridge.

The tendency of copper to forms flattened in the direction of a normal to an octahedral plane is well known and has been clearly brought out by Prof. E. S. Dana, in an article on the copper of Lake Superior.* These flattened forms are shown by him to be generally twins with the twinning plane an octahedral face. Though owing to the small size and especially the extreme thinness of the tabular crystals in the aventurine glass no direct evidence of twinning, such as reëntrant angles on the edges, is to be seen, yet from analogy with the copper crystals described by Dana and others it is probable that they are also twins. Vogt† expresses the opinion that the plates are simple distorted crystals, but gives no reasons for this view.

* Dana, On the Crystallization of Native Copper. This Journal, xxxii, 1886, p. 413.

† Op. cit., p. 238, note 2.

As will be seen presently twinned forms are common among the smaller crystals.

The copper crystals making up the second group are smaller than the preceding, varying from 0·01–0·03^{mm} in diameter, and are not only quite different in habit but much more diverse in crystalline form. They may be divided into three distinct types, each being about equally abundant; cubo-octahedral forms, octahedra, and twins.

To the first of these generally belong the largest individuals and their form is that of the combination of the cube and octahedron, with both about equally developed, or more rarely the combination of the dodecahedron with these, the individuals being evenly developed and showing no signs of distortion or flattening. In addition to these an occasional trisoctahedral plane with other indeterminate forms are to be seen as replacements of the angles of the combinations above. The symbols of the trisoctahedra could not be determined by measurement, but they are tetragonal and are presumably 211 and 311. These crystals though, rarely show sharp edges, the forms being much rounded and only the octahedral and cubic planes remaining flat in most cases; this rounding being occasionally carried so far that the crystals assume an almost spherical shape. The crystals of this type are occasionally partially surrounded by irregular trichitic growths.

The crystals of the second type of this group are rather smaller than the preceding, varying in diameter from 0·01–0·02^{mm}. They are in almost every case perfectly sharp and ideally symmetrical octahedra, cubic and dodecahedral replacements of the angles and edges being rare, and when present very small.

The third type, that of twinned forms, while of about the same size as those just described offer much more variety. The twinning plane is in every case the usual one, an octahedral face, and the twins are either simple or repeated. The simple forms are composed of trigonal cubic twins (fig. 9), and octahedral twins with cubic planes on the angles (fig. 10), both of which are much flattened parallel to the twinning plane. These forms are identical with some described and figured by Dana in the paper already referred to.*

The polysynthetic twins are either fourlings or fivelings (figs. 11 and 12), formed of four or five octahedra grouped about a common center, the first showing a reëntrant angle at one side, and the last forming almost symmetrical pentagonal bi-pyramids, since the octahedral angle (70° 32′) is almost one-

* Loc. cit., pp. 424 and 426, Pl. 11, fig. 28, Pl. 12, fig. 41.

fifth of 360°. Similar forms have been described by v. Lasaulx* and W. G. Brown.†

It has already been said that the tabular phenocrysts occur in a vitreous groundmass which is almost absolutely free from smaller crystals and microlites. The same is true in a certain way of these smaller phenocrysts which have been just described. They occur generally in parts of the glass that are free from the tabular crystals but very thickly strewn with the minute microlites which form the third group, being immediately surrounded, however, by a more or less broad circular zone (Hof) of perfectly clear glass basis, exactly as is seen to be the case with magnetite and other minerals in volcanic rocks. These microlites which are extremely small, running from 0·005–0·0005^{mm} or even less, are seen under high powers to be sharp and perfectly symmetrical octahedra. Forms other than octahedra were not seen even with the use of the highest powers, and the angles are only rarely replaced by cubic planes.

The imperfect glass shows under the microscope the same colorless glass basis as the perfect, but is almost absolutely free from the tabular crystals, only one or two being seen in the slide, and these very small. The copper crystals are in an overwhelming majority the microlites of the third group, which are exactly like those just described. Crystals of the second group are rare and these mostly of the simple octahedral type, with a few cubo-octahedra and scarcely any twins.

The thin greenish blue top layer already spoken of, which is sharply distinct from the brown glass beneath, but which throws projections into the latter, offers some special peculiarities. The surface is, as has been said, covered with very minute pits and small indented stellate forms with generally six rays, then crossing in most cases at about 60°, but in others at irregular angles, and some stars having fewer than six rays. I had a slide from this prepared by Messrs. Voigt and Hochgesang, this smooth original surface being used as one face, while the other was ground down parallel to it.

With low powers under the microscope the centers of these stars are seen to be occupied by holes, and the rays are seen to be less regular than they appear to the naked eye or with the lens. They are seen to be composed of long groups and clusters of black opaque trichites which are either straight or curved. With these are sometimes associated small irregular specks of a pale brown non-pleochroic mineral, which shows marked double refraction. Its precise nature could not be determined, but it seems to be augite. In the pale blue ground-

* Lasaulx, Ber. nied. Ges., xxxix, 1882, 95.

† W. G. Brown, this Journal, xxxii, 1886, p. 377.

mass of glass, which is much cracked, are many smaller clusters of the same trichites.

The blue color of this layer is obviously due to the oxidizing action of the atmospheric air on the copper, analogous to the action of the oxidizing flame of the blowpipe on a borax bead containing copper, and the black trichites may be with great probability supposed to be Tenorite, CuO.

Although some writers have thought that the crystals described in the preceding pages are not metallic copper but a silicate of copper or Cu_2O, yet Wöhler's original determination has been since confirmed,* and their color, luster, opacity, crystalline form and habit, and twinning forms leave no doubt of their nature, even apart from chemical tests.

Vogt has already pointed out the fact that the copper has evidently crystallized from solution in the molten glass exactly like a salt from water, and this view is confirmed by such facts as the blue upper layer, the presence of the clear zones around the crystals of the second group, as well as the fact that they seem to follow the laws of crystal growth in solutions as laid down by O. Lehmann.†

This author shows by numerous examples that crystals tend to grow most rapidly at the most sharply pointed parts, and this we find beautifully exemplified in the projections at the angles of the hexagonal and triangular plates as well as in the salient ridges along the thin edges. Also in accordance with this law is the generally observed fact that the projections on the triangles are on the whole relatively longer than those on the hexagons.

He also shows‡ that the crystal habit varies with the concentration and temperature of the solution and with the rapidity of crystallization, the more irregular forms, such as skeleton crystals, being produced by greater concentration and quicker crystallization among other conditions. The formation of twins seems to be due to disturbing influences on the regular crystallization.§

On such grounds the diverse habits of the various crystal groups are to be explained; the tabular phenocrysts being first formed from a more concentrated solution, some of these, as the conditions changed, tending to grow by additions at the angles and edges, while more copper crystallized out as the smaller phenocrysts, till finally in the last stages only the minute regular octahedral microlites were formed.

The manufacture of this glass being a trade secret I could extract no information from the foreman who gave me the

* Vogt, op. cit., p. 237, note 5.
† Molecular physik. Leipzig, 1888, i, pp. 337 ff.
‡ Op. cit., p. 303. § Op. cit., p. 416.

specimens as to the process or details of its manufacture; a fact which is greatly to be regretted since, for instance, a knowledge of the circumstances to which the failure in the case of the imperfect glass was due would throw light on the crystallization of the copper.

It seems that the glass is produced by melting together glass, cuprous oxide and some reducing agent, such as siderite,* and that FeO is in this case the reducing agent is shown by the greenish color of the imperfect glass, which is not the blue green of copper but the yellow green of ferrous glass, and perhaps due to too large a quantity of reducing agent. This part of the subject, however, as well as the chemical composition of the glass I must for the present leave aside.

Venice, Italy, Aug. 25, 1894.

ART. LX. — *On the Cæsium-Cobalt and Cæsium-Nickel Double Chlorides, Bromides and Iodides;* by G. F. CAMPBELL.

As a continuation of the work in this laboratory on double halogen salts, the investigation of the above-mentioned compounds has been taken up. The study has been made in a systematic manner with the view of preparing as complete a series as possible. The salts obtained belong to three types, and are as follows:

3:1 Type.	2:1 Type.	1:1 Type.
Cs_3CoCl_5	Cs_2CoCl_4	$CsCoCl_3 . 2H_2O$
Cs_3CoBr_5	Cs_2CoBr_4	
	Cs_2CoI_4	
		$CsNiCl_3$
		$CsNiBr_3$

The results show that cobalt forms double salts with much greater facility than nickel, for with the latter metal only the chloride and bromide of a single type could be obtained.

The series illustrates the increase in ease of formation of double salts from the iodides to the chlorides, which has been previously observed, especially in the case of the cæsium-magnesium salts by Wells and Campbell.† No cæsium-nickel iodide could be prepared.

It should be noticed that the two salts of the 3:1 type are exceptions to Remsen's law concerning this class of bodies.

* Wagner-Fischer, Handb. d. Chem. Tech., 1889, pp. 707 and 741, quoted by Vogt.

† This Journal, xlvi, 432.

The previously described double halogen salts of cobalt and nickel, as far as I have been able to learn, correspond to two types of the cæsium salts, and are as follows:

2:1 Type.	1:1 Type.
$(NH_4)_2CoF_4 . 2H_2O$	$NH_4CoCl_3 . 6H_2O$
$(NH_4)_2NiF_4 . 2H_2O$	$NH_4NiCl_3 . 6H_2O$
	$KCoF_3 . H_2O$
	$KNiF_3 . H_2O$
	$NaCoF_3 . H_2O$
	$NaNiF_3 . H_2O$

The following table gives approximately the composition of the solutions from which the cæsium salts under consideration were crystallized by concentration and cooling.

Cs : Co or Ni (Atoms.)

Cs_3CoCl_5	From	12 : 1	to	6 : 1
Cs_2CoCl_4	"	6 : 1	"	0·4 : 1
$CsCoCl_3 . 2H_2O$	"	0·4 : 1	"	syrupy solution of $CoCl_2$
Cs_3CoBr_5	"	2 : 1	"	1 : 1
Cs_2CoBr_4	"	1 : 1	"	syrupy solution of $CoBr_2$
Cs_2CoI_4	"	1 : 4	"	1 : 16
$CsNiCl_3$	"	12 : 1	"	syrupy solution of $NiCl_2$
$CsNiBr_3$	"	2·5 : 1	"	syrupy solution of $NiBr_2$

More or less of the corresponding halogen acid was present in each case, and an increase of this was apparently equivalent in effect to the addition of the cæsium halide. In the case of the two nickel salts, a rather large amount of the acid was desirable, for if it was not present, the salts appeared only upon heating the concentrated solutions and dissolved when they cooled.

The color of the chlorides containing cobalt is a magnificent blue, the bromides and the iodides containing the same metal are green, while the two nickel salts are yellow. The two nickel salts form almost microscopic crystals. The two salts of the 3 : 1 type were obtained in crystals having a diameter of about 5^{mm}, apparently combinations of the cube and octahedron. The salts of the 2 : 1 type form large plates or prisms, the habit evidently depending upon the composition of the solutions from which they crystallize. The salt $CsCoCl_3 . 2H_2O$ forms rather small plates. Besides the blue salt just mentioned, a red cæsium-cobalt chloride of the 1 : 1 type was obtained which lost water of crystallization so readily, with change of color, that it could not be analyzed in its original condition.

The compound Cs_2CoI_4 is deliquescent, while the other salts, here described, are stable. All the salts are whitened when brought into contact with water or alcohol, evidently on account of decomposition.

The following analyses were made:

	Cs_3 Co Cl_5. Cæsium.	Cobalt.	Chlorine.
Found	62·79	9·16	27·83
Calculated	62·82	9·24	27·74

	Cs_2 Co Cl_4. Cæsium.	Cobalt.	Chlorine.
Found	56·86	12·53	30·40
Calculated	56·99	12·58	30·43

	Cs Co Cl_3. $2H_2O$. Cæsium.	Cobalt.	Chlorine.	Water.
Found	38·64	17·67	32·07	10·94
Calculated	39·80	17·56	31·87	10·77

	Cs_3 Co Br_5. Cæsium.	Cobalt.	Bromine.
Found A	46·65	6·88	46·33
" B	----	7·44	46·97
" C	45·81	7·08	46·52
Calculated	46·52	6·84	46·64

	Cs_2 Co Br_4. Cæsium.	Cobalt.	Bromine.
Found A	41·21	9·49	49·43
" B	----	9·20	49·60
" C	----	9·25	49·32
Calculated	41·26	9·10	49·64

	Cs_2 Co I_4 Cæsium.	Cobalt.	Iodine.
Found A	29·69	7·10	----
" B	----	7·34	60·24
" C	----	7·31	60·29
Calculated	31·93	7·09	60·98

	Cs Ni Cl_3. Cæsium.	Nickel.	Chlorine.
Found A	44·42	19.70	35·78
" B	----	19·14	35·57
Calculated	44·61	19·66	35·73

	Cs Ni Br_3. Cæsium.	Nickel.	Bromine.
Found A	29·93	13·83	55·84
" B	30·60	13·58	55·49
Calculated	30·81	13·58	55·61

The author takes pleasure in expressing his indebtedness to Prof. H. L. Wells for valuable advice in connection with this investigation.

Sheffield Scientific School,
New Haven, Conn., September, 1894.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Electric Conductivity of Gases.*—The question whether gases can conduct electrification, has been investigated by Braun, under various conditions. He examined first, compound gases at the moment of their formation, using (1) mixtures of nitrogen dioxide and air and (2) mixtures of chlorine and hydrogen; their union being effected by the influence of heat or of diffuse daylight. For the conductivity measurements, a Leyden battery was employed, having an electromotive force of about 4000 volts. No conduction was detected. In the second set of experiments the gases were examined during the time of explosion, a mixture of carbon monoxide and oxygen being employed, the electromotive force used being that of from seven to twenty Grove cells. Under these conditions, there appeared to be unmistakable conduction through the exploding gas. The last experiments were made with gases heated to high temperatures, about 1000°—1200°, in porcelain tubes, the electromotive force being that of a single Leclanche cell. It was observed that ammonium chloride and cadmium iodide conducted well, ammonia, iodine and hydrogen chloride conducted fairly, while hydrogen iodide, hydrogen bromide and mercuric oxide (probably) conducted only very slightly. No conduction at all could be observed with carbon dioxide, water, or nitrogen tetroxide. In concluding his paper the author expresses the opinion that the electric charges of flames are due to the separation of positive and negative ions. —*Zeitschr. physikal. Chem.*, xiii, 155, February, 1894. G. F. B.

2. *On the connection between the Atomic masses of metals and the Crystallographic characters of their Isomorphous salts.*—A comparative crystallographic study of the normal sulphates of potassium, rubidium and cæsium has been made by Tutton with a view to determine the relation between the atomic masses of these metals and the crystallographic characters of their isomorphous salts. After an elaborate summary of the results obtained the author concludes that "a regular progression is observed in the crystallographical characters of the three sulphates under consideration, corresponding to the progression in the atomic weight of the metal which they contain. With regard to every property whether morphological or physical, the rubidium salt stands intermediate between the potassium and the cæsium salt. The replacement of rubidium by cæsium, however, is generally accompanied by a greater modification of the crystallographical characters than when potassium is replaced by rubidium, the heavier atom exerting an influence greater than in mere simple proportion to the increase in atomic weight. The comparative results for the different properties are all in line with each other, the relations of the amounts of change along the directions of the three crystallo-

graphic axes being similar with respect to all. Moreover the progression according to the atomic weight of the alkali metal is perceptible with regard to the minutest details of the physical phenomena presented by the crystal and is entirely independent of the temperature. It may therefore be finally stated that *the whole of the crystallographical properties of the strictly isomorphous, rhombic, normal sulphates of potassium, rubidium and cæsium, are functions of the atomic weight of the metal which they contain.*"—*J. Chem. Soc.*, lxv, 628–717, July, 1894.

G. F. B.

3. *On the Influence of Moisture on Chemical Change.*—It is well known that certain chemical reactions can take place only in presence of the vapor of water. BAKER has extended the number of these reactions and has investigated the conditions under which they take place. His first experiment was made with lime and sulphur trioxide. The lime was prepared by the ignition of precipitated chalk mixed with sugar. While still warm it was placed in a small bulb blown on a tube, at the other end of which some phosphorus pentoxide was placed. The ends of the tube were sealed; and after three days, the bulb, which had been heated at intervals to drive any moisture into the pentoxide, was sealed off and introduced into one end of a carefully dried glass tube, some phosphorus pentoxide being placed in the other. This tube after exhaustion was sealed and allowed to stand for two days. One end of it was then connected with a tube containing sulphur trioxide, by means of a joint made of previously ignited asbestos, the point of the sealed end was broken and the trioxide was distilled into the tube. After a week, this portion of the tube containing the pentoxide was sealed off and the bulb containing the lime was broken. Not a sign of combination appeared, though on breaking the tube and allowing a little moist air to enter, combination at once took place and the mass became vividly incandescent. On repeating the experiment with black copper oxide in place of lime, the sulphur trioxide crystallized on the black powder, apparently without converting any of it into the white anhydrous copper sulphate. The action of lime upon ammonium chloride was next examined. This was of interest because water is a product of the reaction. No ammonia gas was evolved on mixing them when the materials were perfectly dried. A mixture of dry hydrogen and dry chlorine did not explode when exposed to sunlight; and more than a quarter of the mixture remained uncombined after exposure to diffused sunlight for two days and to direct sunlight for two days. Nitrogen dioxide and oxygen, when pure and dry, do not react on each other. Carefully dried hydrogen chloride and ammonia gases do not combine on being mixed. And conversely perfectly dry ammonium chloride does not undergo dissociation when heated to 350°. As to the explanation of these results, the author is inclined to the physical view of the matter suggested by Vernon Harcourt rather than to the chemical one proposed by Dixon. Indeed he has long

believed in an electrochemical theory of combination; and hence considered it desirable to ascertain whether molecules capable of combining are at different potentials and whether this difference of potential increases as they are brought nearer to the point of union. Moreover, whether also the conditions which affect chemical change affect in the same direction the passage of electric discharge. A mixture of dry hydrogen chloride and dry ammonia in equal volumes was introduced into a glass tube about a meter long, divided in the middle by a threeway tap, and provided at the ends with electrodes of platinum plate. On opening the tap and connecting the electrodes with the terminals of a Wimshurst machine giving 3 inch sparks in air, for three hours, it was found that a separation of the gases had taken place; the gas surrounding the anode reddening litmus and that about the kathode blueing it. Air, dried over sulphuric acid, showed after electrification 1·8 per cent more oxygen at the anode end of the tube; and a mixture of dried hydrogen and oxygen showed 2·3 per cent excess of oxygen at this end. Since then mixed gases can be partially separated by the attraction of their molecules for oppositely charged plates, it seems probable that the molecules themselves are charged. Moreover experiment showed that the electric discharge takes place more readily in moist than in dry air; and further that the electric glow obtained by shaking mercury in different rarefied gases gradually diminishes as the gas is dried by phosphorus pentoxide and finally disappears altogether. Hence the author concludes that with regard to the presence of moisture, electric discharge is affected in the same way as chemical combination. If it can be regarded as proved that substances which are capable of chemically combining are electrically charged, the great significance of this result is obvious.—*J. Chem. Soc.*, lxv, 611–624, July 1894. G. F. B.

4. *On the Production of free Fluorine by Chemical means.*—The isolation of fluorine by Moissan it will be remembered, was effected by means of electrolysis. Brauner has now succeeded in obtaining this gas by a method purely chemical. He had observed twelve years ago, that the compounds CeF_4H_2O and $(KF)_3(CeF_4)_2(H_2O)_2$ when heated, first evolve water and then a gas having the odor of hypochlorous acid and possessing the property of setting iodine free from potassium iodide. He now describes a new series of analogous salts, the fluorplumbates, derivations of fluorplumbic acid; the first member of the series having the composition $(KF)_3 . HF . PbF_4$, and being produced in one of three ways: (1) by treating the oxide $Pb_3O_7 . (H_2O)_3$ freshly precipitated, with a mixture of hydrogen-potassium fluoride and hydrogen fluoride and crystallizing from hydrogen fluoride; (2) by fusing lead dioxide with potassium hydroxide in the proportion $(KOH)_3 : PbO_2$ in a silver crucible, dissolving the moistened mass in excess of hydrogen fluoride and crystallizing; and (3) by dissolving three gram-molecules of $KF . HF$ in excess of hydrogen fluoride and adding one gram-molecule of lead tetrace-

tate, and evaporating; the acetyl being thus displaced by fluorine. The substance is obtained in the form of needle shaped crystals, often a millimeter in diameter and a centimeter long, grouped radially and probably monoclinic, analogous to the fluostannate of Marignac. It is permanent in dry air, but turns brown in moist air, being decomposed by water so as to yield hydrated lead peroxide, hydrogen potassium fluoride, and hydrogen fluoride. Heated to 100°–110° for some hours the mass remains constant; but at 200° hydrogen fluoride is evolved, and at higher temperatures, but much below a red heat, a gas is set free having the characteristic odor of fluorine, and liberating iodine in large quantity from iodide of starch paper. Small crystals of silicon placed in the open end of the evolution tube burn with vivid incandescence, sometimes with explosive violence, in the issuing gas. There can be no doubt that this gas is really fluorine. If the potassium fluorplumbate loses only its hydrogen fluoride at 230°, with traces only of fluorine, one gram should give 47^{cc} of fluorine; the admixed hydrogen fluoride being removed by passing the gas over potassium fluoride. If the sodium salt now under investigation proves to be $(NaF)_2PbF_4$ it would yield fluorine pure at once. The author also describes the preparation of fluorplumbic acid and lead tetrafluoride.—*J. Chem. Soc.*, lxv, 393–402, June, 1894. G. F. B.

5. *On the Structure of the Flame of Cyanogen.*—The structure of the flame of burning cyanogen has been studied by SMITHELLS and DENT according to the method proposed by the first named chemist in connection with Ingle. The cyanogen was prepared by heating mercuric cyanide and was collected over mercury. When burned in the cone-separating apparatus, the flame at the top of the outer tube consists of two parts, an inner cone-shaped region having a bright peach blossom tint, and an outer or enveloping cone, shading off from a bright blue to a greenish-grey. As air is gradually added the inner cone becomes smaller and soon enters and descends the outer tube, being surrounded by a rosy halo, which disappears and is replaced by a blue one as the amount of air increases. The outer cone retains its place at the top of the outer tube and remains of the same color. More air still, extinguishes the outer cone and causes the inner one to become bluer and its halo greener. Examination of the interconal gases showed the presence of carbon dioxide and monoxide, cyanogen, nitrogen, nitrogen dioxide and tetroxide. Analysis gave CO 25·4, CO_2 0·8, $(CN)_2$ 6·1, N_2 66·9, NO 0·3, NO_2 0·4, when the proportion of air to cyanogen was 3·52 : 1. From their results the authors conclude: (1) that the cones may separate when the ratio of air to cyanogen is 3·3 : 1; (2) that when cyanogen is burning with the minimum quantity of air, carbon monoxide is practically the only oxidation product; (3) that as the air supply increases the quantity of carbon dioxide increases, until it equals one half the volume of the monoxide; (4) that cyanogen is present in the interconal gases

amounting to 7½ per cent with the minimum of air, disappearing as the air supply increases; (5) that small but variable quantities of nitrogen oxides are present. In the inner cone therefore carbon monoxide is the main product, and in the outer one carbon dioxide.—*J. Chem. Soc.*, lxv, 603–610, July, 1894. G. F. B.

6. *Select Methods in Chemical Analysis;* by WILLIAM CROOKES, Third Edition (Longmans, Green, & Co.)—The new edition of this well-known work, though no more bulky than its immediate predecessor, includes the description of many new processes of analysis in place of methods which have been superseded or which have become so widely known that they need no mention in a work which aims merely to bring together novelties. Processes of technical importance only, such, for example as methods of furnace assaying or processes of analysis of iron, steel, and ores of iron, have been displaced because such topics are now fully treated elsewhere. The chief additions have been in the line of electrolytic analysis. Only such methods as have been put to the test in the author's own laboratory are admitted, and it is of course to be expected that many developments of the nine years intervening since the issue of the second edition find no mention. This edition will no doubt be as welcome to practical chemists as was each of the preceding issues in its time.

F. A. G.

7. *An Elementary Manual of Chemistry;* by F. H. STORER, Professor of Agricultural Chemistry in Harvard University, and W. B. LINDSAY, Professor of General and Analytical Chemistry in Dickinson College. Being a Revision and Rewriting of Prof. W. R. Nichol's Abridgment of Eliot and Storer's Manual. New York, Cincinnati and Chicago: American Book Company, pp. 453.—The rapidly increasing rate of progress of the experimental sciences renders necessary frequent issue of newly posted text-books, and among the many serviceable handbooks of chemistry, none is likely to be found superior to this "lineal descendant" of Eliot and Storer's Manual whose appearance nearly thirty years since first rendered systematic laboratory instruction to large classes in general chemistry practicable and pleasant both to student and teacher. The original Manual was rather bulky for the most advantageous use. In Nichol's abridgment we thought much of the excellence of the Manual was sacrificed and are glad to find in this less condensed revision and rewriting the touch of master hands evident throughout.

The book is equally valuable in the class room and the laboratory. The instructor will find in it the essentials of chemical science developed in easy and appropriate sequence, its facts and generalizations expressed accurately and fully but concisely as well as forcibly and elegantly. A large number of those fundamental or important facts of chemistry that are inaccessible through ordinary experience are demonstrated in 258 experiments mostly so simple and so well described and illustrated (by 125 engravings) that any intelligent youth provided with the cheap

outfit catalogued on pp. 428–431, may without other aid, make them an intimate part of his personal knowledge. On this surest foundation the demonstrated laws and current hypotheses of the science are skilfully constructed and displayed,

The short Introduction of three pages in four paragraphs with a single experiment defines the province of chemistry, Chapter I in 5 pages, 11 paragraphs and two experiments, sets forth the material and complex nature of common air and shows the making of oxygen from "mercury rust." Chap. II of 4 pp. in 5 ¶¶ with 2 exp's. develops the properties of oxygen in some detail. In Chap. III of 3 pp. 3 ¶¶ and 1 exp. the isolation of nitrogen is exhibited, its physical and chemical properties and its occurrence in nature are stated and the laws of Charles and Boyle are very clearly enounced. Chap. IV of 13 pp. 19 ¶¶ and 10 exps. gives an admirable exposition of the chemistry and chemical physics of water. The caloric and thermal unit, the laws of definite proportions by weight and volume, molecules, atomic weights, the distinction between elements and compounds, distillation, solution, supersaturation and water of crystallization are very suitably developed, described or demonstrated. Chap. V, 12 pp., 13 ¶¶, 6 exps. treats of hydrogen and its dioxide with explanation of reactions, substitution or replacements, molecular weights, crith, vapor density, gas-diffusion and heat of formation are here explained.

Compounds of Nitrogen; Hydrochloric Acid; The Halogens; Ozone; Sulphur, Selenium and Tellurium, are the subjects of Chapters VI to X. In them are considered incidentally Multiple Proportions, by weight and volume; Acids, Bases and Salts; Distinction of Metals from Non-metals; Nascent State; Valence and Replacing Power; Chemical Calculations; Allotropism; Crystal Systems; Dimorphism; Dissociation.

Chapter XI of 8 pp. is purely doctrinal and is paragraphed as follows: Combination by Volume; Molecular Condition of Elementary Gases; Volumetric Interpretation of Symbols; Variation in the Number of Atoms in the Molecule of the Same Element; Atomic Weight and Specific Heat. The law of Avogadro and that of Dulong and Petit are here set forth.

Chapter XII of 8 pp. and 8 ¶¶ is devoted to Formulas, Empirical and Rational; their Determination; a brief account of Dualistic and Typical formulas, and a fuller statement of the character and uses of Structural or Graphic Formulas.

The next three chapters are occupied respectively with Phosphorus, with Arsenic, Antimony and Bismuth and Silicon and Boron. Four chapters, aggregating 128 pp. are devoted to Carbon and its Compounds. In Chap. XVI the properties of Carbon and of its oxides and sulphides, and the subjects of Combustion and Illumination are fully discussed while decay, putrefaction, fermentation, compound radicals, the blowpipe, and kindling temperatures, are more briefly noticed. Chapters XVII, XVIII and XIX are an excellent epitome of organic chemistry.

The remaining chapters save the last treat of the metals. The work concludes with Chapter XXXII which in seven pages comprises a Table of Atomic Weights wherein hydrogen is the unit and oxygen has the value 15·88 and gives a remarkably lucid exposition of the Periodic Law.

8. *Movements of the Solar Atmosphere.*—M. H. DESLANDRES has discovered by comparison of many photographs of the solar lines H and K a variation in the bright line which is found in the dark space embraced by these lines. This bright line can be separated into two enclosing a dark line. The bright lines correspond to the lower layers of the chromosphere while the dark line belongs to the higher layers. These bright lines often show dissymmetry, sometimes one and sometimes the other becoming the narrower. Spectra of the faculæ do not usually show this dissymmetry but it is a common condition on the remainder of the surface, and is more pronounced near the equator than in the neighborhood of the poles. Near spots the observed dissymmetry is often in the opposite direction on opposite sides, and the narrowing of the line is sometimes irregular. The phenomena can be explained on the hypothesis of a continued circulation of the sun's atmosphere, but it is worth noting that a less marked dissymmetry has been obtained in the calcium spectrum produced by the induction spark. Resemblances are pointed out between these phenomena and those observed in the spectrum of Nova Aurigæ.—*Comptes Rendus*, Aug. 27; *Nature*, Sept. 6, 1894, p. 468. J. T.

9. *Luminous effects produced by electric oscillation.*—H. EBERT describes a luminescence lamp made of a glass globe containing luminous paint. Oscillations are produced on tin foil strips placed on the globe and vivid luminescence is produced. The energy consumed was in the millionths of a watt, and this economy of the lamp was very striking. To avoid the great losses due to inductive resistances a transformer with small capacity and small inductance could be attached directly to the lamp.—*Ann. der Physik und Chemie*, No. 9, 1894, pp. 144–161. J. T.

10. *The resistance of Bismuth in strong magnetic fields.*—Since the resistance of bismuth is increased when it is placed in a magnetic field, it has been suggested that the metal might serve to measure the strength of the field. H. J. B. HENDERSON states that the curve which shows the relation between the resistance and the field intensity was a straight line between a field intensity of 4000 C. G. S. to a field of 15,000 C. G. S. and this relation holds for still more extensive fields. The resistance of a bismuth wire spiral rose at the ordinary normal temperature from 10 ohms with a null field to 33 ohms with a field of 39,000 C. G. S. With lower temperatures the change is relatively greater. With higher temperatures less. It may be that at a certain temperature the change of resistance in the magnetic field disappears. Whether this temperature coincides with the melting point of bismuth can only be decided by experiment.—

Physical Society of Berlin, June 1, 1894; *Ann. der Physik und Chemie*, pp. 57, 58. J. T.

11. *Motion of dielectric bodies in a homogeneous electrostatic field.*—Le Graetz and L. Fomm describe a dielectric volmeter which consists of a thin disc of sulphur or paraffin suspended between the plates of a condenser. The disc tends to place its axis along the lines of electrostatic force. The rotations depend on the size of the charge on the plate, and the oscillations are proportional to the square of the difference of potential.—*Ann. der Physik und Chemie*, No. 9, 1894, pp. 86–94. J. T.

12. *Maxim's flying machine.*—Mr. Hiram S. Maxim gives in the National—an English magazine—a description of his experiments on flying by means of an æroplane. His flying machine when finished and loaded with water, fuel and three men, weighed nearly 8000 lbs. The actual horse power developed on the screws was 363 horse power, with a screw thrust of about 2000 lbs. The total width of the machine was over 200 feet. On running the machine at 30 miles an hour very little load remained on the track and at 36 miles an hour the whole machine was completely lifted.—*Nature*, Sept. 13, p. 489. J. T.

13. *A Treatise on the Measurement of Electrical Resistance.* By William Arthur Price, M.A., A.M.I.C.E. 8°, pp. xvi, 199. Oxford, Clarendon Press, 1894.—This volume is an exposition of the theory and practice of the measurement of electrical resistance, with especial reference to those considerations that arise in actual work, and which are either altogether omitted in ordinary general treatises, or very insufficiently presented. In the first chapter the meaning of resistance, its laws, and units of measurement are considered. This is followed in the second chapter by a discussion of the materials best suited for resistance coils, their temperature coefficients, their variation with time, and attention is drawn to the special qualities of some of the newer materials, as platinoid and manganin. In succeeding chapters the construction of bobbins, the best methods of winding resistance coils, and the precautions to be observed in order to secure accuracy and permanence are taken up. The various forms of commutators and switches, Wheatstone's bridge, the meter bridge in its later modifications, with the various accessories and adaptations, the most suitable arrangements for very low or very high resistance are discussed in detail.

In several Appendices are contained the mathematical theory of Wheatstone's bridge, Lord Kelvin's modification of it for low resistances, electromotive forces at the junctions of meter bridges, the discharge of a condenser through a high resistance, Mance's method of measuring the resistance of a battery, and the electrostatic analogue of Wheatstone's bridge.

The pages of the book contain many interesting remarks and practical suggestions derived from experience, and will be a valuable aid to those studying the subject in a general way, and

especially to those engaged in the construction of standard coils and instruments, or in the actual work of electrical measurement.

A. W. W.

14. *Practical Work in General Physics.* By W. G. Woollcombe. Crown 8°, pp. xii, 83. Oxford, Clarendon Press. 1894. —This is a collection of some fifty practical exercises, with descriptions of instruments and methods for the measurement of lengths, areas, volumes, and densities. Other topics are the barometer, the simple pendulum, capillarity and surface tension. The theory of the balance and the methods of determining weights are briefly indicated, with practical directions sufficient to prepare the experimenter for such use of the balance as is required in many of the exercises. The experiments, which are mostly of rather simple character, are clearly described with considerable detail, and, for the limited range of subjects, the book will be a useful guide to the student. A. W. W.

II. Geology and Mineralogy.

1. *An Occurrence of Anorthite and Epidote* (communicated). —In a large series of specimens collected at Phippsburg, Maine, by T. F. Lamb of Portland, these two minerals occur under unusual circumstances. The mass of the material consists of cinnamon garnet, with occasional green pyroxene, similar to the well-known occurrences at Raymond, Maine, and elsewhere, the rock itself being evidently a highly metamorphosed contact limestone. Occasionally there is imbedded in the masses of garnet a dark gray mineral, nearly black, in brilliant plates, in nowise suggesting epidote. This however, it proved to be, after partial analysis by Dr. W. F. Hillebrand, and optical examination by Mr. J. S. Diller. The anorthite, also verified optically by Mr. Diller, and by partial analysis by Mr. George Steiger, is associated sometimes with the epidote, and sometimes in coarse crystals, only with the garnet. The analyses are as follows:

	Epidote.	Anorthite.
SiO_2	38·54	45·62
Al_2O_3	28·39	35·29
Fe_2O_3	6·89	
FeO	·50	
CaO	24·12	17·31
MgO	trace	
Ignition	2·26	
	100·70	98·22

On first inspection I supposed the epidote to be axinite, which is said to occur at the Phippsburg locality, and which in this case it somewhat resembles. F. W. Clarke.

2. *A new locality of true Emeralds* (communicated).—In July, 1894, a new locality of true emeralds was discovered, by Mr. J.

L. Rorison, miner of mica, and Mr. D. A. Bowman, on the Rorison property near Bakersville, Mitchell County, N. C. Here, at an elevation of five thousand feet a. t., on Big Crab-Tree Mountain, occurs a vein of pegmatite some five feet wide, with well-defined walls, in mica schist. This vein carries a variety of minerals besides its component quartz and feldspar, among these being garnets; translucent, reddish, and black tourmalines, the latter abundant in slender crystals; white, yellow, and pale-green beryls; and the emeralds. These latter are chiefly small, 1 to 10^{mm} wide by 5 to 25^{mm} long, but some have been found two or three times larger than the larger size named. They are perfect hexagonal prisms, generally well terminated, and are clear and of good color, with some promise for gems. They very strikingly resemble the Norwegian emeralds from Arendal.

One vein outcrops for perhaps a hundred yards, with a north to south strike. The results thus far obtained are only from about five feet depth of working, so that much more may be looked for as the vein is developed.

The locality is fourteen miles south of Bakersville, and about the same distance from Mitchell's Peak, a little north of the crest of the Blue Ridge. It is some fifty miles west of the emerald locality at Stony Point, Alexander County, N. C., described by William Hidden in 1881 in a pamphlet privately printed at New York, and in the Transactions of the New York Academy of Sciences, 1882, pp. 101–105, as also by the writer in "Gems and Precious Stones of North America," New York, 1888, p. 91.

I am indebted to Messrs. Rorison and Bowman for the information contained in this paper and for the privilege of examining the specimens found by them. GEORGE F. KUNZ.

3. *The Physical Geology and Geography of Great Britain, by the late Sir Andrew C. Ramsay*, 6th edition edited by HORACE B. WOODWARD, pp. 1–421 (Edward Stanford), London, 1894.—This simple account of English geology prepared in 1863, originally as a course of lectures to workingmen, after having passed through five editions and now revised to express the more modern views of geology still retains the charm which came from the personal enthusiasm of its author, a prominent figure in English geology a generation ago.

4. *The Story of our Planet*, by T. G. BONNEY, pp. 1–535 (The Cassell Publishing Co.)—The author of this treatise has attempted, as he says in his preface, to tell the story of our planet in fairly plain words and has framed the book on a plan similar to that adopted by Sir C. Lyell in his great work "The Principles of Geology," the last edition of which was published twenty-one years ago. Some of the more difficult problems of the science have been left out of discussion, and particular attention has been given to some subjects, in which his personal interests have been concerned. Among these latter are the physical geography of Britain in the earlier part of the Triassic,—the affects and former extent of glaciers and the history and age of certain crystalline

rocks. The book is illustrated and is a thoroughly readable book for general students interested in the scientific discussions of the day.

III. Botany.

1. *Flora of Mount Desert Island, Maine. A Preliminary Catalogue of the plants growing on Mount Desert and the adjacent Islands;* by Edward L. Rand and John H. Redfield. Cambridge: John Wilson & Son. 1894.—The territory covered by this catalogue possesses no special botanical interest except that which attaches to its proximity to the sea, and to the retention of some of the characteristic glacial plants. But these two features have attracted the attention of a group of assiduous explorers who now embody in this convenient and pleasing volume some of the results of their studies.

With the exception of the Fungi and some other Cryptogams, the catalogue is considered by the authors to represent fairly the existing knowledge regarding the species in their limits. The authors have had the assistance of collaborators who have worked up special genera, and they have made extensive use of local notes of a trustworthy character contributed by many observers. To add to the value of the handbook, an interesting article by Prof. W. M. Davis, on the geology of the region, has been given. This article is remarkable for its vigorous and attractive style.

At the outset, the authors declare their position with respect to proposed changes in nomenclature. After expressing the belief in which all must share, that a catalogue ought to be made as useful as possible to all who employ it, the joint authors waive certain personal preferences and adopt substantially the names as given in Gray's Manual. They then state with great force their reasons for not accepting the so-called Rochester and Madison codes. These reasons are essentially two: (1) these codes do not in themselves possess elements of permanency, and (2) they disregard the principle that *ex post facto* legislation is inherently wrong. As a clear and judicial charge to the jury of botanists who have not yet prejudged the case, the communication made by the authors of the catalogue may be cordially commended.

G. L. G.

2. *Researches in regard to the respiration of Muscineæ.*—B. Jönsson (Comptes rendus, 20 Aug., 1894).—The following is a summary of the results. In mosses there is a wide range of variation as regards the intensity of respiration and chlorophylline assimilation, and this variation depends largely on the content of water. Specimens of the same species taken from a very wet place emitted more gas than did those selected from dry places. Reddish color diminishes both respiration and the activity of assimilation.

G. L. G.

3. *On the Constitution of the Atmosphere* (Comptes rendus, 20 Aug., 1894). — T. L. Phipson adds further suggestions to his

earlier one that the primitive vegetation was essentially anaërobic, gradually adapting itself to the changing conditions by which the amount of oxygen in the atmosphere was augmented and heat diminished, and becoming thus partly aërobic. Looking at cells, he traces their course from primitive vegetal forms of an aërobic type up to the animal cell which is strictly aërobic. The most interesting fact in the paper is quoted from an earlier communication, by which it appears that the confined atmosphere, in which a plant of *Convolvulus arvensis* grew, contained, after three months, a larger percentage of oxygen than is found in our atmosphere. G. L. G.

IV. Astronomy and Mathematics.

1. *The Collected Mathematical papers of Henry John Stephen Smith*, edited by J. W. L. Glaisher, with a mathematical introduction by the editor, biographical sketches, and a portrait. Two vols., 4°, pp. xcv, 603 and vii, 719. Clarendon press, Oxford, 1894.—These volumes contain 47 papers published by Prof. Smith, and one memoir on the Theta and Omega Functions not before published. All but three or four of the papers have appeared in the publications of learned societies or in the scientific journals and therefore could be consulted by those who had access to large libraries. But to all others they were as a whole practically inaccessible. The volumes are designed as a memorial, and in the character of the work presented and in this form of publication they constitute a magnificent and most worthy memorial of a remarkable man.

Professor Smith's earliest work was in pure Geometry, but very soon he was attracted to the theory of numbers, and his six reports on that subject made to the British Association are well known to mathematicians. In following years he became interested in elliptic functions. The longest paper in the second of these volumes, that on the Theta and Omega functions, grew out of a request to him from Prof. Glaisher that he would write an introduction to the volume of Tables of the Theta Functions, which had been computed by Prof. Glaisher in connection with a Committee of the British Association. For some unexplained reason these tables have not yet been given to the public. H. A. N.

2. *Cordoba Durchmusterung Atlas containing the positions and brightness of all the fixed stars down to the 10·0 magnitude for the mean equinox of 1875·0 in the belt of the heavens comprised between 22 and 42 degrees of southern declination;* to accompany vols. xvi and xvii of results of the Argentine National Observatory, published by the Observatory, 1893. Also vol. xvii of Results.—These twelve maps with the Argelander and Schönfeld maps cover the whole heavens down to 42° southern declination. This Durchmusterung and Atlas formed part of Dr. Gould's plan of work in going to Cordoba, but was deferred by him in order to carry out his meridian circle observations. It

has been undertaken by his successor Director Thorne. The maps are much more crowded with stars as Mr. Thorne observed stars down to the 10th magnitude. Vol. xvi was distributed some months ago, and now comes vol. xvii completing the catalogue of the zone. H. A. N.

V. Miscellaneous Scientific Intelligence.

1. *Popular lectures and addresses*, by Sir William Thomson (Baron Kelvin), in three volumes, Vol. II, *Geology and general Physics*, pp. 1–599 (Macmillan & Co.) 1894.—This is the final (published) volume of the valuable series of opinions, publically expressed on numerous cases of general interest involving the laws of pure science in the realms of physics, astronomy, geology and kindred branches, by this clear and calm judge of modern science. Some of the more important subjects upon which decisions are given in the present volume are: The doctrine of Uniformity in Geology; the Annual loss of Heat from the Earth; Geological time; the Origin and amount of Plutonic Energy; the Meteoric theory of the Sun's heat; on the Physical Condition of the Earth; the internal condition of the Earth, as to temperature, fluidity, and rigidity; Polar ice-caps and their influence in changing sea levels; on the origin of motive power, and natural sources of energy, the dissipation of energy, etc. There are also numerous addresses, résumés of opinions of others and of progress of science. The articles have been published in various places, and were originally delivered at various times ranging from 1856 to 1893, but lose nothing of their value by republication. In their present form they become accessible to the general student of science as a body of valuable scientific decisions which will stand until a more exhaustive analysis of the facts shall be made.

2. *The International Geological Congress* held its sixth meeting at Zurich from August 29th to September 3d, under the presidency of Professor Renevier, with an attendance of over 220 members. A large number of valuable papers were read of both general and local importance. A report on the state of progress of the geological map of Europe was made by Dr. Hauchecorne of Berlin. Six of the forty-nine projected sheets are now ready, including those for the northwest part of Europe, northern Germany, parts of France, Belgium, Poland, etc. The map is being published by Dietrich Reimer, Berlin.

Michel Levy, in an address before the Congress, proposed that some general system of classification of rocks be adopted by petrographers and suggested that it be founded primarily upon *texture* and secondarily upon the essential *constituent minerals*. A commission was appointed to consider the proposition and to revise the present classification and nomenclature of rocks.

M. E. de Margerie, Secretary of the Commission on Bibliography announced that the "Catalogue of Geological Bibliographies" prepared by the Commission and now in press will be sent

gratuitously to all members of the last and of the present Congress.

The guide book, prepared by the local *comité d'organisation* of the Congress, forms an admirable geological description of the important features of the Swiss Jura and Alps. It is a volume of 306 pages entitled "*Livret-guide géologique dans le Jura et les Alpes de la Suisse*," with numerous woodcuts and thirteen colored maps, illustrating the itineraries of the eleven excursions planned for the members, and was prepared by several of the ablest Swiss geologists. This guide book, together with a beautiful new geological map of Switzerland, on a scale of 1 : 500,000, brings before geologists the latest detailed scientific interpretation of this always interesting region of the Alps.

The next meeting of the Congress, the seventh, will be held in 1897 at St. Petersburg. Arrangements are being made for a particularly interesting meeting in Russia, excursions are planned to cross Russia and the Urals into Siberia, and one to the Caucasus and the Caspian Sea. The Czar has expressed his interest in the Congress by inviting it to meet in St. Petersburg, and by subscribing liberally toward defraying the expenses of the Congress at its Russian meeting.

3. *Chloride of mercury batteries.*—Mr. D. H. FITCH of Cazenovia, N. Y., wishes attention called to the fact that he made use of the chlorides of mercury as the active agents in chemical batteries prior to the date of Helmholtz's description cited in Professor Carhart's article in this Journal.* Mr. Fitch took out a United States patent for a battery constructed on this principle, dated Sept. 16, 1879, the application for which was filed Sept. 9, 1878, entitled No. 219,631, Galvanic Batteries, Derick H. Fitch.

L'or a Minas Geraes (Brézil), by PAUL FERRAND, Vol. I, pp. 159, Ouro Preto, 1894.

Minnesota, Geological and Natural History Survey, Vol. III.

The Lower Silurian Ostracoda of Minnesota, by E. O. ULRICH, pp. 629–693. 1894.

The Lower Silurian Trilobites of Minnesota, by JOHN M. CLARK, pp. 694–759. 1894.

The Animal as a Machine and a Prime Motor, and the laws of Energetics, by R. H. THURSTON, pp. 1–97 (John Wiley & Sons). 1894.

Congrès géologiques international, Livret-guide géologique dans le Jura et les Alpes de la Suisse. (Comité d'Organisation, VI Session, Zurich), pp. 1–306. 89 figures, i–xiii plates. 1894.

Arkansas, Annual Report of the Geological Survey for 1892, Vol. II, *The Tertiary Geology of Southern Arkansas*, by G. D. HARRIS, pp. 1–207. 1894.

Miscellaneous Reports, pp. 1–349. 1894:

F. W. SIMONDS and T. C. HOPKINS: The Geology of Benton County.
J. C. BRANNER: Elevations in the State of Arkansas.
J. C. BRANNER: Observations on erosion above Little Rock.
J. C. BRANNER: Magnetic observations.
F. A. SAMPSON: The Mollusca of Arkansas.
C. H. BOLLMAN: The Myriopoda of Arkansas.
SETH E. MEECH: The Fishes of Arkansas.
C. E. SIEBENTHAL: The Geology of Dallas County.
J. C. BRANNER: Bibliography of the Geology of Arkansas.

* A one-volt standard cell. This Journal, vol. xlvi, p. 60.

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